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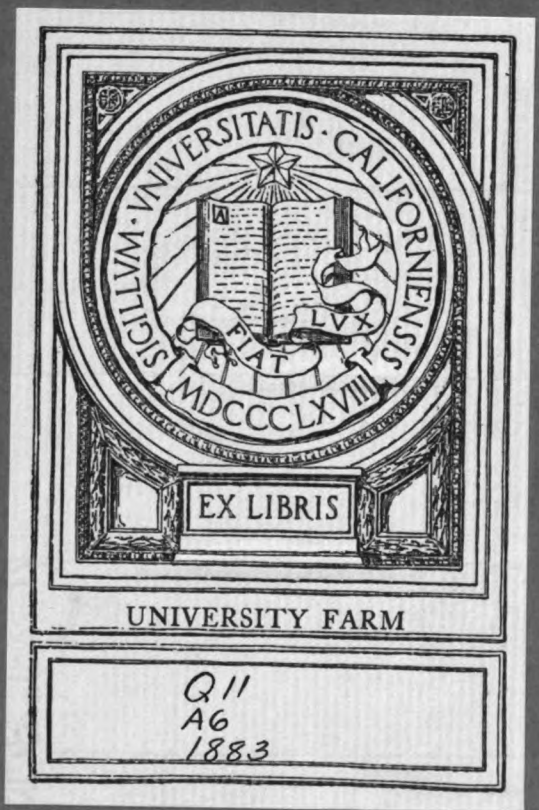

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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

THIRTY-SECOND MEETING,

HELD AT

MINNEAPOLIS, MINN.,

AUGUST, 1883.

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THE SALEM PRESS,

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Hon. George A. Brackett, Hon. A. C. Rand, Hon. John De Laittre, W. W. McNair, Esq., Hon. John S. Pillsbury, Dr. W. W. Folwell, Hon. Charles W. Johnson, Gen. A. B. Nettleton, Hon. W. D. Washburn, Mr. T. B. Walker, Hon. O. C. Merriman, Hon. Eugene M. Wilson, Mr. E. V. White, Mr. H. T. Welles, Hon. H. G. Hicks, Thomas Lowry, Esq., Mr. Winthrop Young, Hon. William S. King, David Blakely, Esq., Hon. R. B. Langdon, Supt. D. L. Kiehle, I. C. Seeley, Esq., Mr. Anthony Kelly, Dr. A. F. Elliott, Hon. F. W. Brooks.

COMMITTEE OF ALDERMEN FROM THE CITY COUNCIL.

Chairman.— W. M. GLENN.

F. L. Greenleaf, E. Eichhorn, N. H. Roberts, Matthew Walsh, E. M. Johnson, S. P. Channell.

SUB-COMMITTEE ON INVITATIONS AND RECEPTION.

Chairman.— Dr. W. W. FOLWELL.

Mr. D. C. Bell, Hon. E. M. Wilson, Mr. Samuel Hill, Hon. C. M. Loring, Hon. C. A. Pillsbury, Hon. A. A. Ames (Mayor), Hon. J. B. Gilfillan, Rev. J. H. Tuttle, Hon. H. G. Hicks, Gen. A. B. Nettleton, Rev. James McGolrick, Bishop Cyrus D. Foss, Mr. C. McC. Reeve, Prof. M. Marston, Dr. George F. French, Dr. C. L. Wells, V. S. Ireys, Esq., Rev. D. B. Knickerbacker, Mr. R. E. Grimshaw, Hon. A. C. Rand, S. C. Gale, Esq., W. W. McNair, Esq., Rev. C. A. Van Anda, Mr. O. A. Pray, Judge C. E. Vanderburg, Mr. B. F. Nelson, Robert S. Innes, Esq., Dr. H. H. Kimball, I. C. Seeley, Esq., Mr. R. J. Mendenhall, Mr. S. A. Harris, Prof. S. Oftedal, Dr. W. H. Leonard, Mr. C. F. Hatch, Col. Fred. Hooker, Judge William Lochren, Hon. D. Morrison, Hon. N. F. Griswold, Mr. J. W. Griffin, Mr. T. B. Cary, Rev. H. C. Woods, J. B. Atwater, Esq., Mr. W. E. Burwell, Mr. Winthrop Young, Rev. Robert Forbes, Col. R. C. Benton, Judge John P. Rea, Rev. R. F. Sample, Col. William McCrory, Mr. G. A. Wheaton, Mr. J. T. Elwell, Mr. C. H. Prior, Mr. T. F. Andrews, Hon. R. B. Langdon, Mr. J. N. Nind, Hon. R. Chute, Mr. George H. Christian, Rev. R. G. Hutchins, Judge A. H. Young, Mr. George A. Camp, Mr. Clinton Morrison, Judge J. M. Shaw, Capt. J. C. Whitney, Hon. Loren Fletcher, Mr. John Crosby, Judge G. B. Cooley, Mr. J. H. Clark, Prof. Jabez Brooks, Mr. W. H. Hinkle, Capt. J. N. Cross, Mr. G. H. Clinton, Mr. A. C. Loring, Hon. C. W. Johnson, Hon. O. C. Merriman, and Aldermen W. M. Glenn, S. P. Channell, Matthew Walsh, N. P. Roberts, and E. M. Johnson.

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LADIES RECEPTION COMMITTEE.

Chairman.— Mrs. J. S. Pillsbury.

Mrs. Richard Chute, Miss Maria Sanford, Mrs. N. H. Winchell, Mrs. W. W. Folwell, Mrs. J. B. Gilfillan, Miss Addie Pillsbury, Miss Emily McMillan, Mrs. L. W. Campbell, Mrs. F. C. Barrows, Miss Lettie Crafts, Miss Addie Camp.

SUB-COMMITTEE ON ROOMS AND PLACES OF MEETING.

Chairman.— Hon. E. M. Wilson.

Hon. R. B. Langdon, Hon. C. W. Johnson, Mr. Anthony Kelly, Dr. A. F. Elliott, Hon. C. M. Loring, Prof. J. A. Dodge.

SUB-COMMITTEE ON ENTERTAINMENT, HOTELS, LUNCHEONS AND LODGINGS.

Chairman.— Hon. A. C. Rand.

Mr. R. F. Jones, Stephen Mahoney, Esq., Hon. George A. Brackett, George H. Fletcher, Esq., Dr. F. A. Dunsmoor, Mr. T. B. Walker, R. S. Innes, Esq., Dr. Charles Simpson, Mr. W. M. Tenny, Fred. Lathrop, Esq., Dr. A. W. Abbott, Mr. Wesley Neil, Mr. J. F. Collum, T. E. Byrnes, Esq., Prof. William J. Warren, Mr. Charles R. Chute, E. Chatfield, Esq., Hon. D. L. Kichle, Prof. J. F. Downey, Mr. C. C. Sturtevant, Dr. P. L. Hatch.

SUB-COMMITTEE ON FINANCE.

Chairman.— I. C. Skeley, Esq.

Mr. H. G. O. Morrison, Mr. N. F. Griswold, Mr. O. T. Swett, J. W. Perkins, Esq., Mr. S. B. Lovejoy, Judge Francis Bailey, Mr. Albert Hastings, Hon. Josiah Thompson, jr., Frank H. Carleton, Esq., Hon. H. T. Welles, Hon. F. W. Brooks, Mr. Isaac McNair, and Aldermen W. M. Glenn and S. P. Roberts.

SUB-COMMITTEE ON PRINTING AND ADVERTISING.

Chairman.— Mr. David Blakely.

Mr. C. A. Nimocks, Gen. A. B. Nettleton, Hon. W. S. King, Prof. C. W. Hall.

SUB-COMMITTEE ON TRANSPORTATION AND EXCURSIONS.

Chairman.— Thomas Lowry, Esq.

Hon. W. D. Washburn, Mr. C. F. Hatch, Mr. E. V. White, Mr. John Crosby, Mr. W. H. Hinkle, Hon. J. S. Pillsbury, Mr. Llewellyn Christian, Hon. J. B. Bassett, Mr. C. H. Prior, Mr. A. H. Bode, Mr. W. H. Truesdale, Col. William McCrory.

SUB-COMMITTEE ON AUDITING ACCOUNTS.

E. M. Loring, Esq., and Aldermen E. Eichhorn and F. L. Greenleaf.

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OFFICERS ELECTED

FOR THE

PHILADELPHIA MEETING.

SEPTEMBER THIRD, 1884.

PRESIDENT.

J. P. LESLEY, of Philadelphia.

VICE PRESIDENTS.

A. Mathematics and Astronomy—H. T. EDDY of Cincinnati.
B. Physics—JOHN TROWBRIDGE of Cambridge.
C. Chemistry—JOHN W. LANGLEY of Ann Arbor.
D. Mechanical Science—R. H. THURSTON of Hoboken.
E. Geology and Geography—N. H. WINCHELL of Minneapolis.
F. Biology—E. D. COPE of Philadelphia.
G. Histology and Microscopy—T. G. WORMLEY of Philadelphia.
H. Anthropology—E. S. MORSE of Salem.
I. Economic Science and Statistics—JOHN EATON of Washington.

PERMANENT SECRETARY.

F. W. PUTNAM of Cambridge.

GENERAL SECRETARY.

ALFRED SPRINGER of Cincinnati.

ASSISTANT GENERAL SECRETARY.

EDWARD S. HOLDEN of Madison.

SECRETARIES OF THE SECTIONS.

A. Mathematics and Astronomy—G. W. HOUGH of Chicago.
B. Physics—N. D. C. HODGES of Salem.
C. Chemistry—R. B. WARDER of North Bend.
D. Mechanical Science—J. BURKITT WEBB of Ithaca.
E. Geology and Geography—EUGENE A. SMITH of Tuscaloosa.
F. Biology—C. E. BESSEY of Ames, Iowa.
G. Histology and Microscopy—ROMYN HITCHCOCK of New York.
H. Anthropology—W. H. HOLMES of Washington.
I. Economic Science and Statistics—C. W. SMILEY of Washington.

TREASURER.

WILLIAM LILLY of Mauch Chunk.

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MEMBERS OF THE STANDING COMMITTEE

FOR THE

PHILADELPHIA MEETING.

Past Presidents.—JAMES D. DANA of New Haven; JAMES HALL of Albany; ISAAC LEA of Philadelphia; F. A. P. BARNARD of New York; J. S. NEWBERRY of New York; B. A. GOULD of Boston; T. STERRY HUNT of Montreal; ASA GRAY of Cambridge; JOSEPH LOVERING of Cambridge; J. E. HILGARD of Washington; SIMON NEWCOMB of Washington; O. C. MARSH of New Haven; GEORGE F. BARKER of Philadelphia; GEORGE J. BRUSH of New Haven; J. W. DAWSON of Montreal; C. A. YOUNG of Princeton.

Vice Presidents of the Last Meeting.—W. A. ROGERS of Cambridge; H. A. ROWLAND of Baltimore; EDWARD W. MORLEY of Cleveland; DE VOLSON WOOD of Hoboken; C. H. HITCHCOCK of Hanover; W. J. BEAL of Lansing; J. D. COX of Cincinnati; O. T. MASON of Washington; F. B. HOUGH of Lowville.

Officers of the Present Meeting.—J. P. LESLEY of Philadelphia; H. T. EDDY of Cincinnati; JOHN TROWBRIDGE of Cambridge; JOHN W. LANGLEY of Ann Arbor; R. H. THURSTON of Hoboken; N. H. WINCHELL of Minneapolis; E. D. COPE of Philadelphia; T. G. WORMLEY of Philadelphia; E. S. MORSE of Salem; JOHN EATON of Washington; F. W. PUTNAM of Cambridge; ALFRED SPRINGER of Cincinnati; EDWARD S. HOLDEN of Madison; G. W. HOUGH of Chicago; N. D. C. HODGES of Salem; R. B. WARDER of North Bend; J. BURKITT WEBB of Ithaca; EUGENE A. SMITH of Tuscaloosa; C. E. BESSEY of Ames; ROMYN HITCHCOCK of New York; W. H. HOLMES of Washington; C. W. SMILEY of Washington; WILLIAM LILLY of Mauch Chunk.

From the Association at Large.—A fellow to be elected from each Section.

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SPECIAL COMMITTEES.

1. *Permanent Committee on Weights, Measures and Coinage.*

F. A. P. BARNARD of New York,
B. A. GOULD of Boston,
J. E. HILGARD of Washington,
H. A. NEWTON of New Haven,
E. B. ELLIOTT of Washington,
JOHN TROWBRIDGE of Cambridge,
A. M. MAYER of Hoboken.

2. *Committee on Membership.*

THE MEMBERS OF THE STANDING COMMITTEE.

3. *Committee on the best methods of Science-Teaching in the Public Schools.*

E. L. YOUMANS of New York,
A. R. GROTE of New York,
J. S. NEWBERRY of New York,
J. W. POWELL of Washington,
N. S. SHALER of Cambridge,
N. T. LUPTON of Nashville.

4. *Committee on the Registration of Deaths, Births, and Marriages.*

E. B. ELLIOTT of Washington,
J. B. KILLEBREW of Nashville,
E. T. COX of San Francisco,
F. B. HOUGH of Lowville,
J. S. COPES of New Orleans,
JOHN COLLETT of Indianapolis.

5. *Committee on Stellar Magnitudes.*

E. C. PICKERING of Cambridge,
LEWIS BOSS of Albany,
S. W. BURNHAM of Chicago,
ASAPH HALL of Washington,
WILLIAM HARKNESS of Washington,
E. S. HOLDEN of Madison,
SIMON NEWCOMB of Washington,
C. H. F. PETERS of Clinton,
ORMOND STONE of Univ. of Va.
C. A. YOUNG of Princeton.

6. *Committee to confer with Committees of Foreign Associations for the Advancement of Science with reference to an International Convention of Scientific Associations.*

T. STERRY HUNT of Montreal,
ALEXANDER AGASSIZ of Cambridge.
SIMON NEWCOMB of Washington.

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7. *Committee on Indexing the Literature of the Chemical Elements.*

H. CARRINGTON BOLTON of Hartford,
IRA REMSEN of Baltimore,
F. W. CLARK of Washington,
A. R. LEEDS of Hoboken,
A. A. JULIEN of New York.

8. *Committee on International Congress of Geologists.*

JAMES HALL of Albany,
J. W. DAWSON of Montreal,
J. S. NEWBERRY of New York,
T. STERRY HUNT of Montreal,
C. H. HITCHCOCK of Hanover,
RAPHAEL PUMPELLY of Newport,
J. P. LESLEY of Philadelphia.

9. *Committee to confer with the United States Geologist in regard to coöperation between the Government and State Geological Surveys.*

JAMES HALL of Albany,
T. STERRY HUNT of Montreal,
J. S. NEWBERRY of New York,
N. H. WINCHELL of Minneapolis,
C. H. HITCHCOCK of Hanover,
J. P. LESLEY of Philadelphia,
G. H. COOK of Brunswick,
E. A. SMITH of Tuscaloosa,
JOSEPH LE CONTE of San Francisco.

This Committee has power to add to its number.

10. *Committee on Interchange of Courtesies between the American and British Associations for the Advancement of Science.*

C. A. YOUNG of Princeton,
J. P. LESLEY of Philadelphia,
J. W. DAWSON of Montreal,
T. STERRY HUNT of Montreal,
E. S. MORSE of Salem,
T. C. MENDENHALL of Columbus,
F. W. PUTNAM of Cambridge.

11. *Committee in relation to Duty on Scientific Books.*

H. A. ROWLAND of Baltimore,
C. A. YOUNG of Princeton,
J. W. POWELL of Washington,
J. D. COX of Cincinnati,
E. S. MORSE of Salem.

12. *Auditors.*

HENRY WHEATLAND of Salem,
THOMAS MEEHAN of Germantown.

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MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr., C. B. Trego,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,	J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,*		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

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MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5 *	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,*	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,*		S. St. John, 7	S. F. Baird,	A. L. Elwyn.
8th	April 26 1854,	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,*	Joseph Lovering,	J. L. LeConte.*8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857,	Montreal, Canada,	Alexis Caswell,* 9	Alexis Caswell,*	John L. LeConte,*	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,*	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.*
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,*	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.*
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.*
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,*	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam,	W. S. Vaux.†*

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

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MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION OF CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION OF MICROSCOPY.	CHAIRMAN OF PERMANENT SUBSECTION OF ENTOMOLOGY.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,*	———	———
25th	Aug. 23, 1876.	Buffalo, N. Y.,	W. B. Rogers,*	C. A. Young,	E. S. Morse,	G. F. Barker,	L. H. Morgan,*	R. H. Ward,	———
26th	Aug. 29, 1877.	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,†	R. H. Ward,	———
27th	Aug. 21, 1878.	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	———[2]	R. H. Ward,[3]	———
28th	Aug. 27, 1879.	Saratoga, N. Y.,	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,	———
29th	Aug. 25, 1880.	Boston, Mass.,	L. H. Morgan,*	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,	———
30th	Aug. 17, 1881.	Cincinnati, Ohio,	G. J. Brush,	Wm. Harkness,[7]	E. T. Cox,[8]	G. C. Caldwell,[9]	G. Mallery,	A. B. Hervey,	J. G. Morris.

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.	SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.	TREASURER.
F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	———	———	W. S. Vaux.*
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	———	W. S. Vaux.*
F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	———[2]	T. O. Summers, jr.,	———	W. S. Vaux.†*
F. W. Putnam,	H. C. Bolton,	E. E. Nipher,	George Little,	A. P. S. Stuart,	———[2]	G. J. Engelmann,	———	W. S. Vaux.†*
F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.†*
F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.*
F. W. Putnam,	C. V. Riley,	E. T. Tappan,[10]	Wm. Saunders,	A. Springer,[12]	J. G. Henderson,	W. H. Seaman,[11]	B. P. Mann,	W. S. Vaux.†*

[1] In the absence of E. C. Pickering.
[2] The Subsection united with Sec. B.
[3] In the absence of G. S. Blackie.
[4] In the absence of Ira Remsen.
[5] In the absence of George Little.
[6] In the absence of A. C. Wetherby.
[7] In the absence of A. M. Mayer.
[8] In the absence of George Engelmann.
[9] In the absence of W. R. Nichols.
[10] In the absence of John Trowbridge.
[11] In the absence of S. P. Sharples.
[12] In place of H. W. Wiley, called away.
* Deceased.
† Not present.

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MEETINGS AND OFFICERS OF THE ASSOCIATION. (*Continued.*)

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENTS.				
				Section A.	Section B.	Section C.	Section D.	Section E.
31st	Aug. 23, 1882.	Montreal, Can.	J. W. Dawson,	W. A. Rogers,[1]	T.C. Mendenhall,	H. C. Bolton,	W.P.Trowbridge,	E. T. Cox,
32nd	Aug. 15, 1883.	Minneapolis, M.	C. A. Young,	W. A. Rogers,	H.A.Rowland,	E. W. Morley,	De Volsen Wood,[7]	C.H. Hitchcock,

VICE PRESIDENTS.				Permanent Secretary.	General Secretary.	Assistant Gen. Secretary.	SECRETARIES OF SECTIONS.	
Section F.	Section G.	Section H.	Section I.				Section A.	Section B.
W. H. Dall,	A. H. Tuttle,	Alex. Winchell,[2]	E. B. Elliott,	F. W. Putnam,	Wm. Saunders,	J. R. Eastman,	H. T. Eddy,	C. S. Hastings,
W. J. Beal,	J. D. Cox,	O. T. Mason,	F. B. Hough,	F. W. Putnam,	J. R. Eastman,	Alfred Springer,	G. W. Hough,[8]	F. E. Nipher,[9]

SECRETARIES OF THE SECTIONS.							Treasurer.
Section C.	Section D.	Section E.	Section F.	Section G.	Section H.	Section I.	
Alfred Springer,	J. B. Webb,[3]	H. S. Williams,[4]	Wm. Osler,[5]	Robt. Brown, jr.,	O. T. Mason,	F. B. Hough,[6]	——————*
J. W. Langley,[10]	J. B. Webb,[11]	A. A. Julien,	S. A. Forbes,	Carl Seiler,	G. H. Perkins,	Jos. Cummings,	William Lilly.

[1] In the absence of William Harkness.
[2] In the absence of Daniel Wilson.
[3] In the absence of C. B. Dudley.
[4] In the absence of C. E. Dutton.
[5] In the absence of C. S. Minot.
[6] F. B. Hough was called home and J. R. Dodge acted at the close of the meeting.
[7] Absent but place was not filled.
[8] In the absence of W. W. Johnson.
[9] In the absence of C. K. Wead.
[10] W. M. McMurtrie, last three days of meeting.
[11] Pro tempore.
*Deceased.

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COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be, possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

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CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in General Session.

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in General Session.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Standing Committee and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

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ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected. The Standing Committee shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Committee made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, an Assistant General Secretary, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Standing Committee shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the

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Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee.

ART. 13. The Assistant General Secretary shall be the Secretary of the Standing Committee. He shall give to the Secretary of each Section the titles of papers assigned to it by the Standing Committee.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ one or more clerks at such compensation as may be agreed upon by the Standing Committee.

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ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Assistant General Secretary, and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Standing Committee shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Assistant General Secretary, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A. M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The Committee shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other

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proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; nominate members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association.

ART. 19. The Nominating Committee shall consist of the Standing Committee, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 21. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into nine Sections, namely:—A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to agriculture and the arts;* D, *Mechanical Science;* E, *Geology and Geography;* F, *Biology;* G, *Histology and Microscopy;* H, *Anthropology;* I, *Economic Science and Statistics.* The Standing Committee shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

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ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section nor Subsection until it has been received from the Standing Committee and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Standing Committee with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

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Printed Proceedings.

Art. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles taken from the "title blanks" will appear in the printed volume. The Standing Committee shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

Local Committee.

Art. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

Library of the Association.

Art. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

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Admission Fee and Assessments.

Art. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 35. The annual assessment for members and fellows shall be three dollars.

Art. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Standing Committee.

Art. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

Art. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Standing Committee.

Alterations of the Constitution.

Art. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers entered and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Election of members.
7. Election of fellows.
8. Unenumerated business.
9. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

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MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28).
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).

MEMBERS.[2]

Abbot, S. L., M. D., 90 Mt. Vernon St., Boston, Mass. (29).
Abbott, E. K., M. D., Salinas, Monterey Co., Cal. (29).
Abert, Col. James W., Newport, Ky. (31).
Abert, S. Thayer, 1907 Pennsylvania Avenue, Washington, D. C. (30).
Acker, E. O'C., Ass't Eng'r, U. S. N., Norristown, Pa. (28).
Adams, Frank, 41 McGill College Avenue, Montreal, Can. (29).
Adams, John S., M. D., Oakland, Cal. (29).
Adams, Julius W., 155 Congress St., Brooklyn, N. Y. (29).
Adams, Capt. Robert C., Montreal, Can. (31).
Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).
Agnew, Dr. Cornelius R., 266 Madison Ave., New York, N. Y. (32).
Ainslie, Charles N., Rochester, Minn. (32).
Allen, Edmund T., 404 Market St., St. Louis, Mo. (27).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes bound in cloth, or by the payment of twenty dollars, to the volumes bound in half morocco.*

Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

The names of Patrons are to remain permanently on the list.

[2] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

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Allen, Dr. G. M., 102 Clark St., Cincinnati, Ohio (30).
Allen, J. M., Hartford, Conn. (22).
Alling, J. C., Supt. City Schools, Albert Lea, Minn. (31).
Alling, Miss Mary R., Acra, Greene Co. N. Y. (29).
Allis, Solon M., Tucson, Arizona (29).
Allison, Henry, Fort Worth, Texas (32).
Ammen, Daniel, Rear Admiral U. S. Navy, Washington, D. C. (26).
Anderson, Newton M., Columbus, Ohio (30).
Andrews, L. W., Ph. D., Springfield, Mass. (31).
Angus, William, 214 St. James St., Montreal, Can. (31).
Ansley, Clark F., Swedona, Mercer Co., Ill. (32).
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Apthorp, Henry, Ashtabula, Ohio (32).
Archambault, U. E., P. O. Box 1944, Montreal, Can. (31).
Armstrong, Lucius H., St. Nicholas, Duval Co., Fla. (30).
Armstrong, Mrs. Lucius H., St. Nicholas, Duval Co., Fla. (30).
Arnold, Lauren B., Rochester, N. Y. (30).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Ashmead, Wm. H., Jacksonville, Fla. (29).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwater, Richard M., Millville, N. J. (29).
Atwood, E. S., East Orange, N. J. (29).
Atwood, Oscar, Rutland, Vt. (31).
Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29).
Aughey, Prof. Samuel, Univ. of Nebraska, Lincoln, Neb. (29).
Austin, Miss Mary, Mattapan Station, Milton, Mass. (31).
Ayres, Prof. Brown, Univ. of Louisiana, New Orleans, La. (31).
Ayres, Dr. S. C., 64 W. 7th St., Cincinnati, Ohio (30).

Babbit, John, Fredericton, N. B. (29).
Babbitt, Miss Franc E., P. O. Box 75, Little Falls, Morrison Co., Minn. [(32).
Bailey, E. H. S., Bethlehem, Pa. (25).
Baker, A. L., 32 So. Charles St., Baltimore, Md. (30).
Baker, B. N., 32 So. Charles St., Baltimore, Md. (30).
Baker, Frank, M. D., 326 C St., Washington, D. C. (31).
Baker, Prof. I. O., Illinois Industrial Univ., Champaign, Ill. (30).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Baker, Wm. E., 158 Tremont St., Boston, Mass. (29).
Baldwin, Miss Mary A., Summer Ave., Cor. Kearney St., Newark, N. J [(31).
Baldwin, Very Rev. Dean Maurice S., Montreal, Can. (31).
Balen, Abraham D., Plainfield, N. J. (31).
Ballard, H. H., Lenox, Mass. (31).
Ballou, Nahum E., M. D., Sandwich, Ill. (29).
Ballou, Wm. H., 105 Dearborn Avenue, Chicago, Ill. (29).
Banes, Charles H., 2416 Spring Garden St., Philadelphia, Pa. (31).
Baptie, George, Normal School, Ottawa, Ontario, Can. (29).

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Barclay, Robert, M. D., 14 Fifth Ave., New York, N. Y. (30).
Bardeen Francis L., M. D., Washington College, Chestertown, Md. (32).
Barker, Mrs. Martha M., care of H. R. Barker, 8 and 10 Central St., Lowell, Mass. (31).
Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., Hotel Vendôme, Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barrows, Nathan, M. D., Hartford, Conn. (29).
Bartlett, Arthur K., Battle Creek, Mich. (29).
Bartlett, Dr. Homer L., Flatbush, L. I. (28).
Bassett, Daniel A., Crawfordsville, Ind. (29).
Bassett, George W., M.D., Vandalia, Ill. (20).
Bassnett, Thomas, Box 936, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Box 936, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Bastin, Prof. E. S., Univ. of Chicago, Chicago, Ill. (29).
Batchelor, Charles, Electrician, Menlo Park, N. J. (28).
Batterson, J. G., Hartford, Conn. (23).
Bausch, Edward, Rochester, N. Y. (26).
Bayne, Prof. Herbert A., Royal Military College, Kingston, Ont. (29).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beadle, Delos W., St. Catharines, Ontario, Can. (31).
Beal, F. E. L., Agricultural College, Ames, Iowa (29).
Beall, Thomas, Lindsay, Ontario, Can. (31).
Beamer, Miles (30).
Bean, Thos. E., Box 441, Galena, Ill. (28).
Beard, Richard O., M. D., Minneapolis, Minn. (32).
Beaudry, J. Alphonse U., 97 St. James St., Montreal, Can. (31).
Beauregard, Gen. Gustave T., New Orleans, La. (30).
Bechdolt, Adolphus F., Mankato, Minn. (32).
Beckwith, Henry C., Passed Ass't Eng'r U. S. N., Coleman's Station, N. Y. (29).
Beebe, Wm. S., Brevet Major, U. S. Army, 25 Grace Court, Brooklyn, N. Y. (30).
Belknap, Chas., Lieut. U. S. N., U. S. S. Vandalia, care Navy Dept., Washington, D. C. (29).
Belknap, Morris B., Louisville, Ky. (29).
Belknap, Wm. B., Louisville, Ky. (29).
Bell, Alex. Melville, 88 Fayette St., Georgetown, D. C. (31).
Bell, John J., Exeter, N. H. (22).
Bell, Thomas D., McGill College, Montreal, Can. (31).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y. (25).
Bemrose, Joseph, 158 P. O., Montreal, Can. (31).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19).

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Benjamin, Marcus, 43 East 67th St., New York, N. Y. (27).
Berliner, Emile, 95 Milk St., Boston, Mass. (29).
Best, W. F., St. John, N. B. (29).
Beyer, H. G., Passed Ass't Surgeon, U. S. N., 125 Park Ave., Baltimore, Md. (31).
Bickmore, Mrs. Albert S., 12 East 41st St., New York, N. Y. (31).
Bicknell, Hon. Thos. W., Boston, Mass. (29).
Bigelow, A. T., D. D. S., Bismarck, Dakota (32).
Bigelow, Otis, 605 7th St., Washington, D. C. (30).
Bigelow, Robert P., 1501 18th St., N. W., Washington, D. C. (32).
Bigelow, Wm. Sturgis, M. D., 61 Beacon St., Boston, Mass. (29).
Bill, Charles, Springfield, Mass. (17).
Bingham, Mrs. Martha A., Kansas City, Mo. (32).
Birge, Charles P., Keokuk, Iowa (29).
Blackford, Eugene G., 809 Bedford Avenue, Brooklyn, N. Y. (29).
Blackwell, Mrs. A. B., Somerville, N. J. (30).
Blaisdell, F. E., Poway, San Diego Co., Cal. (29).
Blake, Mrs. Clarence J., 19 St. James Avenue, Boston, Mass. (29).
Blake, Francis C., Penna. Lead Works, Mansfield Valley, Alleghany Co., Pa. (29).
Blake, Prof. John R., Davidson College, N. C. (29).
Blakslee, Prof. T. M., Harpersville, N. Y. (31).
Blanchard, Frederick, Box CC, Lowell, Mass. (29).
Blatchford, Eliphalet W., 875 North La Salle St., Chicago, Ill. (17).
Blatchford, John S., 13 Exchange St., Boston, Mass. (29).
Blitz, Adolph, M. D., Minneapolis, Minn. (32).
Blodgett, A. N., M.D., Boylston St., Boston, Mass. (29).
Blount, Henry F., Evansville, Vanderburg Co., Ind. (32).
Boardman, Miss E. D., 120 Beacon St., Boston, Mass. (29).
Boardman, Mrs. Wm. D., 38 Kenilworth St., Roxbury, Mass. (28).
Bobbitt, Mrs. John, Greensburg, Decatur Co., Ind. (30).
Boerner, Chas. G., Vevay, Ind. (29).
Bolles, Dr. Wm. P., 571 Dudley St., Dorchester, Mass. (29).
Bond, Geo. W., Boston, Mass. (29).
Bookwalter, J. W., Springfield, Ohio (30).
Bourland, Dr. Addison M., Van Buren, Ark. (29).
Bowditch, Frederic C., Box 615, Brookline, Mass. (29).
Bowers, Miss Virginia K., care Wm. Rosselle, Memphis Appeal Office, Memphis, Tenn. (27).
Bowker, W. H., 43 Chatham St., Boston, Mass. (30).
Bowles, George J., 1466 St. Catherine St., Montreal, Can. (31).
Bowles, Miss Margaretta, The Institute, Columbia, Tenn. (26).
Boyd, William, Cambridge, Mass. (29).
Boynton, Miss S. P., Box 150, Lynn, Mass. (19).
Brackett, John W., 581 Washington St., Boston, Mass. (29).
Brackett, Solomon H., St. Johnsbury, Vt. (29).
Bradford, Edward H., M. D., 150 Boylston St., Boston, Mass. (29).

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Bradford, Royal B., Lt. Comd'r, U. S. N., 21 Brinley St., Newport, R. I. (31).
Bradley, C. W., cor. 3d and Walnut Sts., Cincinnati, Ohio (27).
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Mohr, Miss Marie, Cincinnati, Ohio (30).
Mohr, Paul, Cincinnati, Ohio (30).
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Moore, E. C., care Tiffany & Co., New York, N. Y. (30).
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Morison, Rebecca N., Minneapolis, Minn. (32).
Morrice, D., Montreal, Can. (31).
Morrison, Nathan J., Springfield, Mo. (29).
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Osborn, Herbert, Ames, Iowa (32).
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Pettee, Prof. C. H., Hanover, N. H. (31).
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Robertson, Col. Robert S., Fort Wayne, Ind. (20).
Robertson, Thomas D., Rockford, Ill. (10).
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Thompson, W. W., care Jeffras Seely & Co., Cincinnati, Ohio (30).
Thomson, Prof. A., Ames, Iowa (21).
Thomson, Prof. Henry R., Crawfordsville, Ind. (30).
Thorburn, John, LL. D., Ottawa, Ont., Can. (29).
Thrasher, Allen B., M. D., 94 W. 7th St., Cincinnati, Ohio (30).
Thurber, Miss Elizabeth, Plymouth, Mass. (22).
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Timby, Theo. R., Nyack, N. Y. (29).
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Todd, Albert, St. Louis, Mo. (27).
Todd, Andrew J., 261 Broadway, New York, N. Y. (29).
Todd, Prof. James E., Tabor, Fremont Co., Iowa (22).
Tolman, James P., West Newton, Mass. (29).
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Townsend, Joseph, Durham, Ontario, Can. (31).
Traber, Jacob, Cincinnati, Ohio (30).
Tracy, Will W., Detroit, Mich. (30).
Treadwell, Geo. A., Phœnix, Arizona (29).
Treat, Erastus B., 757 Broadway, N. Y. (29).
Trippe, T. Martin, C. E , Howardsville, San Juan Co., Col. (29).
Troop, James, Agricultural College, Lansing, Mich. (31).
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Trowbridge, Luther H., 266 Woodward Ave., Detroit, Mich. (29).
Trowbridge, S. H., Glasgow, Mo. (27).
Tucker, Willis G., M. D., Albany, N. Y. (29).
Tyler, Edward R., Washington, D. C. (31).
Tyler, John M., Amherst, Mass. (29).

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Vasey, George, M. D , Washington, D. C. (32).
Venable, W. H., Station C, Cincinnati, Ohio (30).
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Very, Lt. S. W., care of Navy Dep't, Washington, D. C. (28).
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Vining, Edward P., Omaha, Neb. (32).
Vinton, Mrs. Helena Stallo, Indianapolis, Ind. (30).
Vinton, Lindley, Indianapolis, Ind. (30).
Vodges, A. W., 1st Lt. Art'y U. S. A., Fortress Monroe, Va. (32).
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Wachsmuth, Charles, Burlington, Iowa (30).
Wagner, Ferd., 41 W. 5th St., Cincinnati, Ohio (30).
Wainwright, Ellis, St. Louis, Mo. (29).
Wait, Prof. Chas. E., Rolla, Mo. (29).
Waite, Chas. N., Manchester, N. H. (29).

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Walter, Hugo (30).
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Warren, Wm. J., Minneapolis, Minn. (30).
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White, Prof. Frances E., Woman's Med. College, Philadelphia, Pa. (29).
White, Prof. H. C., Univ. of Georgia, Athens, Ga. (29).
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Whitman, Gilbert P., Manchester, N. H. (30).
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Willets, Joseph C., Skaneateles, N. Y. (29).
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Williams, G. Henry, Baltimore, Md. (30).
Williams, J. Francis, Salem, N. Y. (31).
Williams, Janet W., Newton Lower Falls, Mass. (31).
Williams, P. O., M.D., Watertown, Jefferson Co., N. Y. (24).
Williams, Rev. R. R., Canajoharie, Montgomery Co., N. Y. (28).

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Williamson, Prof. Andrew W., Rock Island, Ill. (32).
Willoughby, Hugh L., Saratoga Springs, N. Y. (28).
Wills, William R., Waltham, Mass. (30).
Willson, Robert W., Box 853, New Haven, Conn. (30).
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Wilmot, Thos. J., Direct U. S. Cable Co., Rye, N. H. (27).
Wilson, C. H., Rugby, Tenn. (30).
Wilson, H. C., Mt. Lookout, Hamilton Co., Ohio (30).
Wilson, Horace E., 1514 10th St. N., Minneapolis, Minn. (32).
Wilson, M. C., Florence, Ala. (26).
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Winston, Prof. Chas. H., Richmond College, Richmond, Va. (30).
Witherbee, Frank S., care Witherbees, Sherman & Co., Port Henry, N. Y. (29).
Wolcott, Mrs. Henrietta L. T., Hotel Vendôme, Boston, Mass. (29).
Wood, Dr. Robert W., Jamaica Plain, Mass. (29).
Wood, Dr. Wm., Portland, Me. (29).
Woodbury, C. J. H., 131 Devonshire St., Boston, Mass. (29).
Woodward, Calvin M., St. Louis, Mo. (32).
Woodward, J. J., M. D., Army Medical Museum, Washington. D. C. (28).
Woodward, Richard W., South Pueblo, Col. (29).
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Youmans, Wm. Jay, M. D., Popular Science Monthly, 549 Broadway, New York, N. Y. (28).
Young, Prof. Andrew H., Hanover, Ind. (30).

Zimmerman, Charles D., Buffalo, N. Y. (30).
Zimmerman, William, Room 22, 142 Dearborn St., Chicago, Ill. (30).
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[1474 MEMBERS.]

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FELLOWS.[2]

Abbe, Prof. Cleveland, Washington, D. C. (16). 1874.
Abbott, Dr. Chas. C., Trenton, N. J. (29). 1883.
Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20). 1874.
Allen, Dr. Harrison, 117 South 20th St., Philadelphia, Pa. (29). 1882.
Allen, Joel A., Mus. Comp. Zoology, Cambridge, Mass. (18). 1875.
Alvord, Benj., Brig. Gen., U. S. Army, 1207 Q St., N. W., Washington, D. C. (17). 1874.
Alvord, Henry E., C. E., Houghton Farm, Mountainville, Orange Co., N. Y. (29). 1882.
Anderson, Dr. Joseph, Waterbury, Conn. (29). 1883.
Anthony, Prof. Wm. A., Cornell Univ., Ithaca, N. Y. (28). 1880.
Appleton, Prof. John H., Brown Univ., Providence, R. I. (22). 1874.
Ashburner, Wm., 1014 Pine St., San Francisco, Cal. (29). 1882.
Atkinson, Edward, 131 Devonshire St., Boston, Mass. (29). 1881.
Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29). 1882.
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Austin, E. P., 46 E. Newton St., Boston, Mass. (18). 1874.

Bailey, Prof. Loring W., University of Fredericton, N. B. (18). 1875.
Bailey, Prof. W. W., Brown University, Providence, R. I. (18). 1874.
Baird, Prof. S. F., Sec'y Smithsonian Inst., Washington, D. C. (1). 1875.
Baker, Marcus, Coast Survey Office, Washington, D. C. (30). 1882.
Ballou, Geo. F., Hartford, Conn. (29). 1881.
Bandelier, Ad. F., Highland, Ill. (25). 1879.
BARKER, PROF. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1875.
Barnard, F. A. P., President Columbia College, New York, N. Y. (7). 1874.
Barnard, Prof. Wm. S., U. S. Dept. Agric., Washington, D. C. (24). 1880.
Bartlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883.
Bartlett, J. R., Comd'r U. S. N., care of C. & G. Survey Office, Washington, D. C. (30). 1882.
Bassett, Homer F., Waterbury, Conn. (23). 1874.
Batchelder, Dr. J. H., Salem, Mass. (18). 1874.
Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875.
Beal, Prof. Wm. James, Agricultural College, Lansing, Mich. (24). 1880.

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date at the end of the line the time when made a Fellow.

When the name is given in small capitals, it designates that the Fellow is also a life member, and is entitled to the Annual Volume of Proceedings.

* Deceased.

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Bell, Samuel N., Manchester, N. H. (7). 1874.
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21). 1880.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Bickmore, Prof. Albert S., American Museum of Natural History, Central Park, New York, N. Y. (17). 1880.
Billings, John S., Surgeon U. S. A., Washington, D. C. (32). 1883.
Blackham, George E., M. D., Dunkirk, N. Y. (25). 1883. [1882.
Blair, Henry W., U. S. C. & G. Survey Office, Washington, D. C. (26).
Blake, Clarence J., M. D., Hotel Berkeley, Boston, Mass. (24). 1877.
Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, Auburndale, Mass. (23). 1874.
Bolton, Dr. H. Carrington, Trinity Coll., Hartford, Conn. (17). 1875.
Borden, Spencer, Fall River, Mass. (29). 1882.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bouvé, Thos. T., Boston Soc. Nat. Hist., Boston, Mass. (1). 1875.
Bowditch, Prof. H. P., Harvard Med. School, Boston, Mass. (28). 1880.
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875.
Breneman, A. A., Ithaca, N. Y. (20). 1880.
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875.
Britton, N. L., Columbia College, New York, N. Y. (29). 1882.
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879.
Brooks, Major Thomas B., Geol. State Surveys, Mich. and Wis., Newburgh, N. Y. (26). 1879.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., care of Yale College Observatory, New Haven, Conn (11). 1874.
Brown, Mrs. Robert, jr., New Haven, Conn. (17). 1874.
Brush, Prof. George J., Yale College, New Haven, Conn. (4). 1874.
Buckhout, W. A., State College, Centre Co., Pa. (20). 1881.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass. (22). 1877.
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Burrill, Prof. T. J., Illinois Industrial Univ., Champaign, Ill. (29). 1887

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Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21). 1875.

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Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
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Case, Col. Theo. S., Ed. Western Review of Science, Kansas City, Mo. (27). 1883. [1882.
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Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., 50th St. cor. 4th Ave., New York, N. Y. (19). 1875. [1882.
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Collier, Peter, Dept. of Agric., Washington, D. C. (29). 1882.
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880.
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Comstock, Milton L., Prof. Math., Knox Coll., Galesburg, Ill. (21). 1874.
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24). 1880.
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Cross, Prof. Chas. R., Mass. Inst. Technology, Boston, Mass. (29). 1880.
Cummings, Rev. Joseph, D.D., Pres. Northwestern Univ., Evanston, Ill. (13). 1874.

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Drown, Prof. Thos. M., Lafayette College, Easton, Pa. (29). 1881
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Dudley, Wm. L., Miami Med. College, Cincinnati, Ohio (28). 1881.
Dunnington, Prof. F. P., University of Virginia, Va. (26). 1880.
Dutton, Capt. C. E., U. S. Ord. Dep't, Washington, D. C. (23). 1875.
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Eaton, Hon. John, U. S. Commiss'r of Education, Washington, D. C. (25). 1883.
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Eustis, Prof. Henry L., Cambridge, Mass. (2). 1874.
Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Henry, Ph. D., St. Johnsbury, Vt. (14). 1874.
Fairchild, H. L., 102 E. 52d St., New York, N. Y. (28). 1883.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1875.
Farmer, Moses G., Torpedo Station, Newport, R. I. (9). 1875.
Farquharson, Dr. Robert James, Des Moines, Iowa (24). 1880.
Ferguson, Maj. Thos. B., Ass't U. S. Fish Commissioner, Washington, D. C. (28). 1881.
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (22). 1881.
Fernald, Prof. M. C., State Agricultural College, Orono, Mo. (22). 1883.
Ferrel, Wm., U. S. Coast Survey Office, Washington, D. C. (11). 1875.
Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874.
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883.
Fletcher, James, Library of Parliament, Ottawa, Can. (31). 1883.
Fletcher, Dr. Robert, Surgeon General's Office, Washington, D. C. (29). 1881.
Flint, James M., Surgeon U. S. N., 42 Dartmouth St., Boston, Mass. (28). 1882.
Foote, Dr, A. E., 1223 Belmont Avenue, Philadelphia, Pa. (21). 1874.
Forbes, Prof. S. A., Normal, Ill. (27). 1879.
Frazer, Dr. Persifor, Ass't Geologist, 2d Geol. Survey of Pa., 917 Clinton St., Philadelphia, Pa. (24). 1879.
Frazier, Prof. B. W., The Lehigh University, Bethlehem, Pa. (24). 1882.
Freeman, H. C., Alto Pass, Union Co., Ill. (17). 1876.
Freeman, Spencer H., Adelkert College, Cleveland, Ohio (29). 1883.
French, Prof. Thomas, jr., Univ. of Cincinnati, Cincinnati, Ohio (30). 1883.
Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880.
Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882.

Gage, Simon Henry, Ithaca, N. Y. (28). 1881.
Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23). 1874.
Gardner, James T., Director N. Y. State Survey, Albany, N. Y. (25). 1879.
Garland, Rev. Dr. L. C., Chancellor Vanderbilt Univ., Nashville, Tenn. (25). 1877.

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Garman, S., Museum Comp. Zoology, Cambridge, Mass. (20). 1874.
Gatschet, Albert S., Bureau of Ethnology, Washington, D. C. (30). 1882.
Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1875.
Gilbert, G. K., Box 240, Salt Lake City, Utah (18). 1874.
Gillman, Henry, 155 Adams Avenue East, Detroit, Mich. (24). 1875.
Gilman, Daniel C., President of the Johns Hopkins University, Baltimore, Md. (10). 1875.
Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1875.
Goldschmidt, S. A., Ph. D., Morse Building, 140 Nassau St., New York, N. Y. (24). 1880.
Gooch, Frank A., U. S. Geological Survey, Newport, R. I. (25). 1880.
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875.
Goode, Prof. G. Brown, Curator National Museum, Washington, D. C. (22). 1874.
Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1879.
Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874.
Gray, Prof. Asa, Botanic Gardens, Cambridge, Mass. (1). 1875.
Gray, Elisha, Ph. D., Highland Park, Ill. (32). 1883.
Green, Traill, M. D., Easton, Pa. (1). 1874.
Grimes, J. Stanley, 406 Sycamore St., Milwaukee, Wis. (17). 1874.
Grote, Aug. R., New Brighton, Staten Island, N. Y. (22). 1875.
Gulley, Prof. Frank A., Agricultural College, Oktibbeha Co., Miss. (30). 1883.
Guyot, Prof. Arnold, Princeton, N. J. (1). 1875.

Hagen, Dr. Hermann A., Mus. Comp. Zool., Cambridge, Mass. (17). 1875.
Hague, Arnold, 10 West 29th St., New York, N. Y. (26). 1879.
Hale, Horatio, Clinton, Ontario, Can. (30). 1882.
Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877.
Hall, Prof. C. W., Univ. of Minnesota, Minneapolis, Minn. (28). 1883.
Hall, Edwin H., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Hall, Prof. James, Albany, N. Y. (1). 1875.
Hamlin, Dr. A. C., Bangor, Me. (10). 1874.
Hanaman, C. E., Troy, N. Y. (19). 1883.
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883.
Harger, Oscar, Yale College Museum, New Haven, Conn. (25). 1879.
Harkness, Prof. Wm., U. S. N., U. S. Observatory, Washington, D. C. (26). 1878.
Harrington, Prof. Mark W., Ann Arbor, Mich. (22). 1875.
Harrison, Dr. B. F., Wallingford, Conn. (11). 1874.
Hasbrouck, Prof. I. E., Rutgers Coll., New Brunswick, N. J. (23). 1874.
Haskins, Rev. David Greene, Cambridge, Mass. (25). 1874.
Hastings, C. S., Johns Hopkins University, Baltimore, Md. (23). 1878.
Hazen, Brig. Gen. Wm. B., Chief Signal Office, U. S. A., Washington, D. C. (30). 1882.
Hedrick, B. S., Exam'r U. S. Pat. Office, Washington, D. C. (19). 1874.

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Henderson, John G., Winchester, Ill. (25). 1879.
Henshaw, Henry W., Care Major J. W. Powell, 910 M St., Washington, D. C. (24). 1877.
Hervey, Rev. A. B., Taunton, Mass. (22). 1879.
Hilgard, Prof. E. W., Univ. of California, Berkeley, Cal. (11). 1874.
Hilgard, Prof. J. E., U. S. Coast Survey, Washington, D. C. (4). 1874.
Hill, Franklin C., P. O. Box 338, Princeton, N. J. (29). 1881.
Hill, George W., Nautical Almanac Office, Navy Department, Washington, D. C. (23). 1874.
Hill, Rev. Dr. Thomas, 738 Congress St., Portland, Me. (3). 1875.
Hill, Walter N., Chester, Pa. (29). 1881.
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882.
Hinrichs, Prof. Gustavus, State University, Iowa City, Iowa (17). 1874.
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874.
Hitchcock, Romyn, 53 Maiden Lane, New York, N. Y. (29). 1881.
Hodges, N. D. C., Salem, Mass. (29). 1882.
Hoffmann, Dr. Fred., 797 Sixth Ave., New York, N. Y. (28). 1881.
Holden, Prof. E. S., Director Washburn Observatory, Univ. of Wisconsin, Madison, Wis. (23). 1875.
Holman, Silas W., Mass. Inst. of Technology, Boston, Mass. (31). 1883.
Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881.
Holmes, Wm. H., Smithsonian Institution, Washington, D. C. (30).
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876. [1883.
Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30). 1883.
Hough, Franklin B., Lowville, N. Y. (4). 1874.
Hough, G. W., Director Dearborn Observatory, Chicago, Ill. (15). 1874.
Hovey, Rev. Horace C., Minneapolis, Minn, (29). 1883.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Montreal, Canada (1). 1874.
Huntington, Prof. J. H., Boston, Mass. (19). 1874.
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875.
Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10). 1874.
Hyde, Prof. E. W., Walnut Hills, Cincinnati, Ohio (25). 1881.

James, Jos. F., 177 Race St., Cincinnati, Ohio (30). 1882.
Jeffries, B. Joy, M. D., 15 Chestnut St., Boston, Mass. (29). 1881.
Jenks, Elisha T., Middleborough, Mass. (22). 1874.
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874.
Jewell, Theo. F., Lt. Comdr. U.S.N., Torpedo Station, Newport, R. I. (25). 1882.
Jillson, Dr. B. C., Illinois Industrial Univ., Champaign, Ill. (14). 1881.
Johnson, Prof. S. W., Sheffield Sci. School, New Haven, Conn. (22). 1874.
Johnson, Prof. W. W., Annapolis, Md. (29). 1881.
Joy, Prof. Charles A., Stockbridge, Mass. (8). 1879.
Julien, A. A., School of Mines, Columbia Coll., New York, N. Y. (24). 1875.

Kedzie, Prof. Robert C., Lansing, Mich. (29). 1881.

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Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882.
Kennan, George, 1318 Mass. Avenue, Washington, D. C. (30). 1882.
Kent, William, Pittsburgh, Pa. (26). 1881.
Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1883.
Kinnicutt, Leonard P., Worcester, Mass. (28). 1883.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.
Kunz, G. F., care Tiffany and Co., New York, N. Y. (29). 1883.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 1882.
Langley, Prof. J. W., Univ. of Michigan, Ann Arbor, Mich. (23). 1875.
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18). 1874.
Lanza, Prof. Gaetano, Mass. Inst. of Technology, Boston, Mass. (29) 1882.
Larkin, Edgar L., New Windsor, Mercer Co., Ill. (28). 1883.
Lattimore, Prof. S. A., Univ. of Rochester, Rochester, N. Y. (15). 1874.
Lawrence, George N., 45 E. 21st St., New York, N. Y. (7). 1877.
Lazenby, Prof. Wm. R., Ithaca, N. Y. (30). 1882.
Lea, Dr. Isaac, 1622 Locust St., Philadelphia, Pa. (1). 1875.
LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881.
Ledoux, Albert R., Ph. D., 17 Cedar St., New York, N. Y. (26). 1881.
Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874.
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Philadelphia, Pa. (2). 1874.
Lewis, Prof. Henry C., E. Washington Lane, Germantown, Pa. (26). 1880.
Lilly, Gen. Wm., Mauch Chunk, Carbon Co., Pa. (28). 1882. Patron.
Lindsley, Dr. J. Berrien, Nashville, Tenn. (1). 1874.
Lintner, J. A., N. Y. State Mus. of Nat. Hist., Albany, N. Y. (22). 1874.
Little, W. C., Albany, N. Y. (22). 1874.
Litton, Abram, Washington University, St. Louis, Mo. (28). 1879.
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 1875.
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.
Lord, Prof. Nat. W., State Univ., Columbus, Ohio (29). 1881.
Loudon, Prof. James, Toronto, Can. (25). 1881.
Loughridge, Prof. R. H., Berkeley, Cal. (21). 1874.
Love, Edward G., School of Mines, Columbia College, New York, N. Y. (24). 1882.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1875.
Lull, Edward P., Captain U. S. N., Navy Yard, Boston, Mass. (28). 1880.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyle, David Alexander, Capt. of Ordnance U. S. A., National Armory, Springfield, Mass. (28). 1880.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1875.
Lyman, Hon. Theodore, Brookline, Mass. (23). 1875.
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

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Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881.
Macfarlane, James, Towanda, Pa. (29). 1882.
Mackintosh, James B., School of Mines, Columbia College, New York, N. Y. (27). 1883.
Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882.
Mallery, Brevet Lieut. Col. Garrick, U. S. Army, Box 585, Washington, D. C. (26). 1879.
Mann, B. Pickman, U. S. Dep't of Agric., Washington, D. C. (22). 1874.
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874.
Mark, Edw. L., 48 Shepard St., Cambridge, Mass. (21). 1879.
Markoe, Geo. F. H., 61 Warren St., Roxbury, Mass. (29). 1881.
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15). 1874.
Martin, Prof. Daniel S., Rutgers Female College, New York, N. Y. (23). 1879.
Martin, Prof. H. Newell, Johns Hopkins Univ., Baltimore, Md. (27). 1880.
Mason, Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayer, Prof. A. M., So. Orange, N. J. (19). 1874.
McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., 821 Michigan Ave., Chicago, Ill. (29). 1881.
McGee, W. J., U. S. Geol. Survey, Washington, D. C. (27). 1882.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McNair, Lt. Commander Antoine R., U. S. N., Box 1071, Saratoga Springs, N. Y. (28). 1882.
McRae, Hamilton S., Supt. of Schools, Muncie, Ind. (20). 1874.
Meehan, Thomas, Germantown, Pa. (17). 1875.
Mees, Carl Leo, Columbus, Ohio (24). 1876.
Mendenhall, Prof. T. C., Columbus, Ohio (20). 1874.
Merriman, C. C., Rochester, N. Y. (29). 1880.
Michelson, A. A., Master U. S. N., Nautical Almanac Office, Washington, D. C. (26). 1879.
Minot, Charles Sedgwick, Roslindale P. O., Boston, Mass. (28). 1880.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882.
Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22). 1874.
Morley, Prof. Edward W., 749 Republic St., Cleveland, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875.
Moses, Prof. Thos. F., Urbana Univ., Urbrna, Ohio (25). 1883.
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22). 1874.
Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13). 1874.
Neale, Arthur T., Ph. D., New Brunswick, N. J. (29). 1882.
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882.

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Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24). 1877.
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia College, New York, N. Y. (5). 1875.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13). 1874.
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, E. L., Ph. D., Central Univ. Richmond, Ky. (28). 1881.
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18). 1875.
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876.
Norton, Prof. W. A., New Haven, Conn. (6). 1875.
Norwood, Charles J., Ass't Kentucky Geological Survey, Russellville, Ky. (26). 1881.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1875.
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 1874.
Ordway, Prof. John M., Mass. Inst. Technology, Boston, Mass. (9). 1875.
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875.
Osborn, Henry F., S. D., Garrisons, N. Y. (29). 1883.
Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22). 1874.
Osler, Prof. William, McGill College, Montreal, Canada (28). 1881.
Owen, Dr. Richard, New Harmony, Ind. (20). 1874.

Packard, Dr. A. S., jr., 115 Angell St., Providence, R. I. (16). 1875.
Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parke, John G., Lt. Col. Corps of Eng'rs, Bv't Maj. Gen. U. S. A., Office of Chief of Engineers, Washington, D. C. (29). 1881.
Parkhurst, H. M., Law Stenographer, 25 Chambers St., New York, N. Y. (23). 1874.
Payne, Prof. Wm. W., Carleton College, Northfield, Minn. (30). 1883.
Peckham, S. F., Providence, R. I. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1875.
Peet, Rev. Stephen D., Clinton, Wis. (24). 1881.
Penhallow, Prof. D. P., McGill College, Montreal, Can. (30). 1882.
Perkins, Prof. George H., Burlington, Vt. (17). 1882.
Perkins, Maurice, Schenectady, N. Y. (15). 1875.
Peter, Dr. Robert, Ky. Geol. Survey, Lexington, Ky. (29). 1881.
Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.
Phillips, A. W., New Haven, Conn. (24). 1879.
Phippen, Geo. D., Salem, Mass. (18). 1874.
Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1875.
Pickering, William H., 84 Mount Vernon St., Boston, Mass. (29). 1883.
Pike, Dr. William H., University College, Toronto, Can. (29). 1881.
Pilling, James C., Box 585, Washington, D. C. (28). 1882.

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Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 615 Walnut St., Philadelphia, Pa. (27). 1882.
Potter, Prof. William B., Washington University, St. Louis, Mo. (25). 1879. [1875.
Powell, Major J. W., U. S. Geologist, 910 M St., Washington, D. C. (23).
Prentiss, D. Webster, M. D., Washington, D. C. (29). 1882.
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875.
Prime, Frederick, jr., Allentown Iron Co., 230 So. 3d St., Philadelphia, Pa. (24). 1875.
Pritchett, Henry S., Observatory, Washington Univ., St. Louis, Mo. (29). 1881.
Procter, John R., Director Ky. Geol. Survey, Frankfort, Ky. (26). 1881.
Pulsifer, Wm. H., St. Louis, Mo. (26). 1879.
Pumpelly, Prof. Raphael, U. S. Geol. Survey, Newport, R. I. (17). 1875.
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.
Putnam, James J., M. D., 63 Marlborough St., Boston, Mass. (28). 1880.
Pynchon, Rev. T. R., Pres. Trinity College, Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.
Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.
Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875.
Redfield, J. H., care A. Whitney & Sons, Philadelphia, Pa. (1). 1874.
Reed, E. Baynes, London, Ontario, Can. (27). 1882.
Rees, Prof. John K., Columbia College, New York, N. Y. (26). 1878.
Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875
Rice, John M., U. S. Naval Academy, Annapolis, Md. (25). 1881.
Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.
Richards, Mrs. Robert H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.
Ricketts, Pierre de Peyster, Ph.D., 10 Cedar St., New York, N. Y. (26). 1880.
Riley, Prof. C. V., U. S. Entomologist, 1700 13th St. N.W., Washington, D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Robinson, Prof. S. W., Univ. of Ohio, Columbus, Ohio (30). 1883.
Rockwell, Gen. Alfred P., Office Great Falls Manufacturing Co., Boston, Mass. (10). 1882.
Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, Pa. (11). 1874.
Rogers, Prof. Rob't E., Jefferson Med. Coll., Philadelphia, Pa. (18). 1874.
Rogers, W. A., Ass't Prof. Harvard College Observ., Cambridge, Mass.

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(15). 1875.
Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879.
Rood, Prof. O. N., Cor. E. 49th St. & Madison Ave., New York, N. Y. (14) 1875.
Roosevelt, Clinton, 11 Wall St., New York, N. Y. (11). 1874.
Rosenspitz, Dr. Alexander, Rabbi, Lock Box 430, Portland, Oregon, (26). 1883.
Ross, Waldo O., 189 Devonshire St., Boston, Mass. (29). 1882.
Rowland, Prof. Henry A., Baltimore, Md. (29). 1880.
Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1875.
Russell, I. C., U. S. Geological Survey, Salt Lake City, Utah (25). 1882.
Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (13). 1875.

Safford, Dr. James M., Nashville, Tenn. (6). 1875.
Sampson, Comdr. W. T., U. S. N., Naval Observatory, Washington, D. C. (25). 1881.
Saunders, William, London, Ontario, Canada (17). 1874.
Schanck, Prof. J. Stillwell, Princeton, N. J. (4). 1882.
Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874.
Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24). 1877.
Scudder, Samuel H., Cambridge, Mass. (13). 1874.
Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, D. C. (23). 1874.
Seiler, Carl, M. D., Philadelphia, Pa. (29). 1882.
Sestini, Rev. B., Woodstock, Md. (29). 1882.
Sheafer, P. W., Pottsville, Pa. (4). 1879.
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.
Sigsbee, Chas. D., Comd'r U. S. N., Navy Dep't, Washington, D. C. (28). 1882.
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.
Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1874.
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19). 1874.
Skinner, Joseph J., 131 Devonshire St., Boston, Mass. (23). 1880.
Smiley, Charles W., Smithsonian Institution, Washington, D. C. (28). 1883.
Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20). 1877.
Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30). 1882.
Smith, Mrs. Erminnie A., 203 Pacific Ave., Jersey City, N. J. (25). 1880.
Smith, Prof. F. H., University of Virginia, Charlottesville, Va. (26). 1880.
Smith, Quintius C., M. D., Box 707, Austin, Texas (26). 1881.
Smith, Prof. S. I., Yale College, New Haven, Conn. (18) 1875.
Smock, Prof. John Conover, Box 282, New Brunswick, N. J. (23). 1879.
Snow, Prof. F. H., Lawrence, Kan. (29). 1881.
Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882.

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Spencer, Prof. J. William, Univ. of Missouri, Columbia, Mo. (28). 1882.
Spencer, John W., Geol., Paxton, Sullivan Co., Ind. (20). 1874.
Springer, Dr. Alfred, Box 573, Cincinnati, Ohio (24). 1880.
Stallo, J. B., Masonic Temple, Cincinnati, Ohio (30). 1882.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
Stephens, W. Hudson, Lowville, N. Y. (18). 1874.
Sternberg, George M., Surgeon U.S.A., Care Surgeon General, U.S.A., Washington, D. C. (24). 1880.
Stevens, W. LeConte, 170 Joralemon St., Brooklyn, N. Y. (29). 1882.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1875.
Stone, George H., Colorado Springs, Col. (29). 1882.
Stone, Mrs. Leander, 3352 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Leander McCormick Observatory, Univ. of Va., Va. (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Story, Wm. E., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874.
Sturtevant, E. Lewis, M. D., Geneva, N. Y. (29). 1882.
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20). 1881.
Swallow, Prof. G. C., Columbia, Mo. (10). 1875.
Swift, Lewis, Ph. D., Rochester, N. Y. (29). 1882.

Tainter, Sumner, 1221 Connecticut Ave., Washington, D. C. (29). 1881.
Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Taylor, Arthur F., Ph. D., Case School of Applied Science, Cleveland, Ohio (29). 1882.
Taylor, Wm. B., Smithsonian Institution, Washington, D. C. (29). 1881.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thomas, Benj. F., Ph. D., Univ. State of Missouri, Columbia, Mo. (29). 1882.
Thomas, Prof. Cyrus, Carbondale, Ill. (30). 1882.
Thurston, Prof. R. H., Stevens Institute, Hoboken, N. J. (23). 1875.
Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881.
Townshend, Prof. N. S., Columbus, Ohio (17). 1881.
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27). 1881.
Trembley, J. B., M.D., Oakland, Alameda Co., Cal. (17). 1880.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25). 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.
True, Fred W., U. S. National Museum, Washington, D. C. (28). 1882.
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.

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Upham, Warren, Nashua, N. H. (25). 1880.
Upton, Winslow, Army Signal Office, Washington, D. C. (29). 1883.

Van der Weyde, P. H., M. D., Box 3619, New York, N. Y. (17). 1874.
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882.
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16). 1875.
Vose, Prof. George L., Mass. Inst. Technology, Boston, Mass. (15). 1875.

Wadsworth, M. Edward, Ph. D., Assistant in Lithology, Museum of Comparative Zoology, Cambridge, Mass. (23). 1874.
Walcott, Charles D., U. S. Geol. Survey, Washington, D. C. (25). 1882.
Waldo, Leonard, S. D., New Haven, Conn. (28). 1880.
Walker, Prof. J. R., Bay Saint Louis, Hancock Co., Miss. (19). 1874.
Walker, Prof. Joseph B. (20). 1874.
Wallace, Wm., Ansonia, Conn. (28). 1882.
Waller, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walling, H. F., Cambridge, Mass. (16). 1874.
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28). 1883.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875.
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
Warder, Prof. Robert B., Lafayette, Ind. (19). 1881.
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18). 1874.
Warren, Cyrus M., Brookline, Mass. (29). 1882.
Warren, Prof. S. Edward, Newton, Mass. (17). 1875.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1875.
Wead, Prof. Charles K., Univ. of Michigan, Ann Arbor, Mich. (23). 1880.
Webb, Prof. J. Burkitt, Ithaca, N. Y. (31). 1883.
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23). 1882.
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (7). 1874.
Wells, Daniel H., Hartford, Conn. (18). 1875.
Westcott, O. S., Maywood, Cook Co., Ill. (21). 1874.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheeler, Capt. Geo. M., U. S. E., Lock Box 93, Washington, D. C. (25). 1879.
Wheeler, Orlando B., Ass't U. S. Lake Survey, Detroit, Mich. (24). 1882.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875.
White, Prof. I. C., Univ. of W. Va., Morgantown, W. Va. (25). 1882.
Whitfield, R. P., American Museum Natural History, 77th St. & 8th Avenue, New York, N. Y. (18). 1874.
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883.
Whitman, Chas. O., care G. F. Leonard, Newton Highlands, Mass. (27). 1880.
Whitney, Solon F., Watertown, Mass. (20). 1874.

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Whittlesey, Col. Charles, 1305 Euclid Ave., Cleveland, Ohio (1). 1875.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875.
Wiley, Prof. Harvey W., Dept. of Agric., Washington, D. C. (21). 1874.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, H. S., Cornell Univ., Ithaca, N. Y. (18). 1882.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, President University College, 117 Bloor St., Toronto, Canada (25). 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1875.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Winsor, Justin, Librarian Harvard Univ., Cambridge, Mass. (29). 1882.
Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29). 1881.
Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875.
Wyckoff, Wm. C., 44 Howard St., New York, N. Y. (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Youmans, Prof. Edward L., New York, N. Y. (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874.

Zentmayer, Joseph, 147 So. Fourth St., Philadelphia, Pa. (29). 1882.

[559 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 2033.

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DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.
Abert, J. J., Washington, D. C. (1). Born in 1785. Died January 27, 1863.
Adams, C. B., Amherst, Mass. (1). Born January 11, 1814. Died Jan'y
Adams, Edwin F., Charlestown, Mass. (18). [19, 1853.
Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.
Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec.
Ainsworth, J. G., Barry, Mass. (14). [14, 1873.
Alexander, Stephen, Princeton, N. J. (1). Born Sept. 1, 1806. Died June 25, 1883.
Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.
Allen, Zachariah, Providence, R. I. (1). Died March 17, 1882.
Allston, R. F. W., Georgetown, S. C. (3). Born April 21, 1801. Died April 7, 1864.
Ames, M. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.
Andrews, Ebenezer B., Lancaster, Ohio (7). Died Aug. 21, 1880, aged 59.
Anthony, Charles H., Albany, N. Y. (6). Died in 1874.
Appleton, Nathan, Boston, Mass. (1). Born Oct. 6, 1779. Died July 14
Armstrong, John W., Fredonia, N. Y. (24). [1861.
Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11,
Aufrecht, Louis, Cincinnati, Ohio (30). [1878.

Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.
Bache, Franklin, Philadelphia, Pa. (1). Born Oct. 25, 1792. Died March 19, 1864.
Bailey, Jacob W., West Point, N. Y. (1). Born April 29, 1811. Died Feb. 26, 1857.
Bardwell, F. W., Lawrence, Kan. (13). Died in 1878.
Barnard, John G., New York, N. Y. (14). Died May 14, 1882.
Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.
Barry, Redmond, Melbourne, Australia (25).
Beck, C. F., Philadelphia, Pa. (1).
Beck, Lewis C., New Brunswick, N. J. (1). Born Oct. 4, 1798. Died April 21, 1853.
Beck, T. Romeyn, Albany, N. Y. (1). Born Aug. 11, 1791. Died Nov. 19, 1855.

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Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.
Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.
Benedict, George W., Burlington, Vt. (16). Born Jan. 11, 1796. Died in 1871.
Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19,1877.
Binney, Amos, Boston, Mass. (1). Born Oct. 18, 1803. Died Feb. 18,
Binney, John, Boston, Mass. (3). [1847.
Blackie, Geo. S., Nashville, Tenn. (26).
Blake, Homer C., New York, N. Y. (28). Born Feb. 1, 1822.
Blanding, William, ———, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.
Bomford, George, Washington, D. C. (1). Born 1780. Died March 25, 1848.
Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.
Briggs, Robert, Philadelphia, Pa. (29). Born May 18, 1822. Died July 24, 1882.
Brigham, Charles H., Ann Arbor, Mich. (17). Born July 27, 1820. Died in Jan., 1879.
Brown, Andrew, Natchez, Miss. (1).
Brown, Horace, Salem, Mass. (27).
Burbank, L. S., Woburn, Mass. (18). [1859.
Burnap, G. W., Baltimore, Md. (12). Born Nov. 30, 1802. Died Sept. 8,
Burnett, Waldo I., Boston, Mass. (1). Died July 1, 1854, aged 27.
Butler, Thomas B., Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, F. A., New York, N. Y. (27). Died in 1879.
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5,
Case, William, Cleveland, Ohio (6). [1880.
Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died Jan. 8, 1877.
Chadbourne, Paul Ansel, Amherst, Mass. (10). Born Oct. 21, 1823. Died Feb. 23, 1883.
Chapman, N., Philadelphia, Pa. (1). Born May 28, 1780. Died July 1, 1853.
Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.
Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec.
Cheney, Miss Margaret S., Jamaica Plain, Mass. (29). [13, 1870.
Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.

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Clark, Henry James, Cambridge, Mass. (13). Died July 1, 1873, aged 47.
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1). Born Sept. 6, 1806. Died Feb. 6, 1873.
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.
Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6). Born Dec. 14, 1794. Died April 9, 1872.
Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.
Cramp, J. M., Wolfville, N. S. (11). Born July 25, 1796. Died Dec. 6, 1881.
Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 13, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.
Croswell, Edwin, Albany, N. Y. (6). Born in May, 1797. Died June 13,
Curry, W. F., Geneva, N. Y. (11). [1871.
Curtis, Josiah, Washington, D. C. (18). Died Aug. 1, 1883.

Dalrymple, E. A., Baltimore, Md. (11). Died Oct. 30, 1881.
Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.
Dean, Amos, Albany, N. Y. (6). Born Jan. 16, 1803. Died Jan. 26, 1868.
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York, N. Y. (1). Born in 1792. Died Nov. 21, 1851.
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1). Born Oct. 25, 1781. Died Dec. 15,
Dexter, G. M., Boston, Mass. (11). [1867.
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Dorr, E. P., Buffalo, N. Y. (25). Died March 28, 1881.
Draper, Henry, New York, N. Y. (28).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Born July 4, 1794. Died June 26, 1869.
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10). Died Aug. 9, 1855, aged 54.
Dunn, R. P., Providence, R. I. (14).

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Easton, Norman, Fall River, Mass. (14). Died Dec. 21, 1872.
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emerson, Geo. B., Boston, Mass. (1). Born Sept. 12, 1797. Died March 4, 1881.
Emmons, Ebenezer, Williamstown, Mass. (1). Born May 16, 1799. Died October 1, 1863.
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2). Born April 11, 1794. Died Jan. 15, 1865.
Ewing, Thomas, Lancaster, Ohio (5). Born Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York, N. Y. (6). Born Oct. 9, 1798. Died June 16 1873.
Feuchtwanger, Lewis, New York, N. Y. (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7). Born Jan. 7, 1800. Died March 8,
Fisher, Mark, Trenton, N. J. (10). [1874.
Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.
Fitch, O. H., Ashtabula, Ohio (7). Died Sept. 17, 1882, in his 80th year.
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born Nov. 26, 1790. Died Jan. 23, 1868.
Ford, A. C., Nashville, Tenn. (26).
Forshey, Caleb G., New Orleans, La. (21). Died in Aug., 1881.
Foster, John W., Chicago, Ill. (1). Born March 4, 1815. Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Born Oct. 17, 1795. Died Feb. 6,
Fox, Charles, Grosse Ile, Mich. (7). [1865.
Frazer, John F., Philadelphia, Pa. (1). Born July 8, 1812. Died Oct. 14,
French, J. W., West Point, N. Y. (11). [1872.

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.
Gavit, John E., New York, N. Y. (1).
Gay, Martin, Boston, Mass. (1). Died Jan. 12, 1850, aged 46.
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10). Born in 1816. Died Jan'y 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gould, Augustus A., Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2). Born June 15, 1787. Died Oct. 24, 1859

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Graham, James D., Washington, D. C. (1). Born in 1799. Died Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in 1808. Died March 10, 1860.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19). Born in 1822. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York, N. Y. (4). Born March 9, 1809. Died January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, S. S., Chickies, Pa. (1). Died Sept. 10, 1880, aged 68.
Hale, Enoch, Boston, Mass. (1). Born Jan. 29, 1790. Died Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (1). Born Jan. 17, 1781. Died May 15, 1858.
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, A. M., Plymouth, Mass. (29).
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1). Died April 20, 1853, aged 57.
Hartt, Charles F., Ithaca, N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Born Jan. 4, 1816. Died May 23, 1874.
Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1). Died July 27, 1866, aged 80.
Hempstead, G. S. B., Portsmouth, Ohio (29). Died July 9, 1883, in his 89th year.
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872. [1875.
Hilgard, Theo. C., St. Louis, Mo. (17). Born Feb. 28, 1828. Died Mch. 5,
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1). Born May 24, 1793. Died Feb. 27, 1864.
Hodgson, W. B., Savannah, Ga. (10). Born 1815.
Holbrook, John E., Charleston, S. C. (1). Born Dec. 31, 1796. Died Sept. 8, 1871.
Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.

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Hopkins, Wm., Lima, N. Y. (5.) Died in March, 1867.
Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Houghton, Douglas, Detroit, Mich. (1). Born Sept. 21, 1809. Died Oct. 13, 1845.
Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, Edward B., Washington, D. C. (2). Born June 15, 1822. Died Oct. 2, 1863.
Hunt, Freeman, New York, N. Y. (11). Born March 21, 1804. Died March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles T., Boston, Mass. (1). Born June 21, 1805. Died Aug. 29, 1880.
James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.
Johnson, Walter R., Washington, D. C. (1). Born June 21, 1794. Died April 26, 1852.
Johnson, William Schuyler, Washington, D. C. (31). Born Sept. 20, 1859. Died Oct. 6, 1883.
Jones, Catesby A. R., Washington, D. C. (8).
Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).
Keely, George W., Waterville, Me. (1). Died in 1878.
Keep, N. C., Boston, Mass. (13). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.
King, Mitchell, Charleston, S. C. (3). Born June 8, 1783. Died in 1862.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died
LaRoche, R., Philadelphia, Pa. (12). [Sept. 14, 1875.
Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died in 1852.
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Le Conte, John L., Philadelphia, Pa. (1). Died Nov. 15, 1883.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8). Born Sept. 8, 1830. Died June
Lincklaen, Ledyard, Cazenovia, N. Y. (1). [27, 1862.

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Linsley, James H., Stafford, Conn. (1). Born May 5, 1787. Died Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.
Logan, William E., Montreal, Canada (1). Born April 23, 1798. Died June 22, 1875.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15). [24, 1872.
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June

Maack, G. A., Cambridge, Mass. (18). [16, 1871.
Mahan, Dennis H., West Point, N. Y. (9). Born April 2, 1802. Died Sept.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10). [27, 1859
Mather, William W., Columbus, Ohio (1). Born May 24, 1804. Died Feb
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
M'Conihe, Isaac, Troy, N. Y. (5).
McFadden, Thomas, Westerville, Ohio. (30). Born Nov. 9, 1825. Died Nov. 9, 1883.
McMahon, Mathew, Albany, N. Y. (11).
Meade, George G., Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.
Minifie, William, Baltimore, Md. (12). Born in 1805. Died Oct. 24, 1880.
Mitchel, O. M., Cincinnati, Ohio (3). Born Aug. 28, 1810. Died Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (2). Died April 2, 1869, aged 76.
Mitchell, Wm. H., Florence, Ala. (17).
Morgan, Lewis H., Rochester, N. Y. (10). Born Nov. 21, 1818. Died Dec. 17, 1881.
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel G., Philadelphia, Pa. (1). Born Jan. 26, 1799. Died May 15, 1851.
Mudge, Benjamin F., Manhattan, Kansas (25). Born Aug. 11, 1817. Died Nov. 21, 1879, aged 62.
Munroe, Nathan, Bradford, Mass. (6). Born May 16, 1804. Died July 8,
Munroe, William, Concord, Mass. (18). Died April 27, 1877. [1866.
Mussey, William H., Cincinnati, Ohio (30). Born Sept. 13, 1818. Died Aug. 1, 1882.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.

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Nicholson, Thomas, New Orleans, La. (21).
Nicollett, Jean N., Washington, D. C. (1). Born July 24, 1786. Died Sept. 11, 1843.
Norton, John P., New Haven, Conn. (1). Born in July, 1822. Died Sept. 5, 1852.
Noyes, J. O., New Orleans, La. (21).
Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.
Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.
Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.
Olmsted, Alexander F., New Haven, Conn. (4). Born Dec. 20, 1822. Died May 5, 1853.
Olmsted, Denison, New Haven, Conn. (1). Born June 18, 1791. Died May 13, 1859.
Olmsted, Denison, jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.
Otis, George Alexander, Washington, D. C. (10). Born Nov. 12, 1830. Died Feb. 23, 1881.

Painter, Jacob, Lima, Pa. (23). Died in 1876.
Painter, Minshall, Lima, Pa. (7).
Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.
Parkman, Samuel, Boston, Mass. (1). Died Dec. 15, 1854, aged 38.
Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.
Peirce, B. O., Beverly, Mass. (18). Died Nov. 12, 1883, aged 71 years.
Peirce, Benjamin, Cambridge, Mass. (1). Born April 4, 1809. Died Oct. 6, 1880.
Perkins, George R., Utica, N. Y. (1). Born May 3, 1812. Died Aug. 22, 1876.
Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.
Perry, John B., Cambridge, Mass. (16). Died Oct. 3, 1872, aged 52.
Perry, M. C., New York, N. Y. (10).
Piggot, A. Snowden, Baltimore, Md. (10).
Plumb, Ovid, Salisbury, Conn. (9). [1870.
Pope, Charles A., St. Louis, Mo. (12). Born May 15, 1818. Died July 6,
Porter, John A., New Haven, Conn. (14). Born March 15, 1822. Died Aug. 25, 1866.
Pourtalès, Louis François de, Cambridge, Mass. (1). Born in 1822. Died July 18, 1880.
Pruyn, John V. L., Albany, N. Y. (1). Born June 22, 1811. Died Nov. 21, 1877.
Pugh, Evan, Centre Co., Pa. (14).
Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.

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Putnam, J. Duncan, Davenport, Iowa. (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.
Redfield, William C., New York, N. Y. (1). Born March 26, 1789. Died Feb. 12, 1857.
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10). Born August 27, 1803. Died February 10, 1861.
Rogers, Henry D., Glasgow, Scotland (1). Born 1809. Died May 29, 1866.
Rogers, James B., Philadelphia, Pa. (1). Born February 22, 1803. Died June 15, 1852.
Rogers, William Barton, Boston, Mass. (1). Born Dec. 7, 1804. Died May 30, 1882.
Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Sager, Abram, Ann Arbor, Mich. (6). Born December 22, 1810. Died August 6, 1877.
Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.
Schley, William, New York, N. Y. (28). Died in 1882.
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Senter, Harvey S., Aledo, Ill. (20). Died in 1875. [10, 1872.
Seward, William H., Auburn, N. Y. (1). Born May 16, 1801. Died Oct.
Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.
Sherwin, Thomas, Dedham, Mass. (11). Born March 26, 1799. Died July 23, 1869.
Silliman, Benjamin, New Haven, Conn. (1). Born August 8, 1779. Died November 22, 1864.
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12). [26, 1880.
Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec.
Smith, J. Lawrence, Louisville, Ky. (1). Born Dec. 16, 1818. Died Oct. 1883.
Smith, J. V., Cincinnati, Ohio (5).
Smith, James Y., Providence, R. I. (9). Born September 15, 1809. Died in 1876.
Smith, Lyndon A., Newark, N. J. (9). Born November 11, 1795. Died December 15, 1865.
Snell, Ebenezer S., Amherst, Mass. (2). Born October 7, 1801. Died in September, 1877. [14, 1866.
Sparks, Jared, Cambridge, Mass. (2). Born May 10, 1789. Died March
Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.
Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.
Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.

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St. John, Joseph S., Albany, N. Y. (28). Died Nov. 23, 1882.
Sullivant, Wm. S., Columbus, Ohio (7). Born Jan. 15, 1803. Died April 30, 1873.
Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born Jan. 20, 1778. Died Oct. 3, 1853.
Taylor, Richard C., Philadelphia, Pa. (1). Born January 18, 1789. Died November 26, 1851.
Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.
Teschemacher, J. E., Boston, Mass. (1). Died Nov. 9, 1853, aged 63.
Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.
Thompson, Alexander, Aurora, N. Y. (1).
Thompson, Zadock, Burlington, Vt. (1). Born May 23, 1796. Died Jan. [19, 1856.
Thurber, Isaac, Providence, R. I. (9).
Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.
Tobin, Thomas W., Louisville, Ky. (30). Died Aug. 4, 1883.
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York, N. Y. (1). Born August 15, 1796. Died March 10, 1873.
Torrey, Joseph, Burlington, Vt. (2). Born Feb. 2, 1797. Died Nov. 26, 1867.
Totten, Joseph G., Washington, D. C. (1). Born August 23, 1788. Died April 22, 1864.
Townsend, Howard, Albany, N. Y. (10). Died in 1867.
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9). Died in 1866.
Troost, Gerard, Nashville, Tenn. (1). Born March 15, 1776. Died Aug. 14, 1850.
Tuomey, Michael, Tuscaloosa, Ala. (1). Born September 29, 1805. Died March 20, 1857.
Tyler, Edward R., New Haven, Conn. (1). Died Sept. 28, 1848.

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).
Vaux, William S., Philadelphia, Pa. (1). Born May 19, 1811. Died May 5, 1882.

Wadsworth, James S., Genesee, N. Y. (2). Born October 30, 1807. Died May 8, 1864.
Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.
Walker, Timothy, Cincinnati, Ohio (4). Born Dec. 1, 1802. Died Jan. 15, 1856.
Walsh, Benjamin D., Rock Island, Ill. (17).

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Wanzer, Ira, Brookfield, Conn. (25). Born April 17, 1796. Died March 5, 1879.

Warren, Governeur Kemble, Newport, R. I. (12). Died Aug. 8, 1882, in the 42d year.

Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.

Waterman, Charles, Wakefield, Eng. (1).

Watson, Samuel, Nashville, Tenn. (26).

Watson, J. Craig, Ann Arbor, Mich. (13). Born Jan. 28, 1838. Died Nov. 23, 1880.

Webster, Horace B., Albany, N. Y. (1). Died Dec. 8, 1843.

Webster, J. W., Cambridge, Mass. (1). Died Aug. 30, 1850, aged 57.

Webster, M. H., Albany, N. Y. (1).

Weed, Monroe, Wyoming, N. Y. (5). Died in 1857.

Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.

Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.

Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.

Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.

Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.

White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.

Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.

Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.

Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.

Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.

Willard, Emma, Troy, N. Y. (15). Born Feb. 23, 1787. Died April 15, 1870.

Williamson, R. S., San Francisco, Cal. (12).

Wilson, W. C., Carlisle, Pa. (12).

Winlock, Joseph, Cambridge, Mass. (5). Born Feb. 6, 1826. Died June 11, 1875.

Woodbury, Levi, Portsmouth, N. H. (1). Born Dec. 22, 1789. Died Sept. 4, 1851.

Woodman, John S., Hanover, N. H. (11). Born in 1819. Died May 15, 1871.

Wright, John, Troy, N. Y. (1).

Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.

Young, Ira, Hanover, N. H. (1). Died Sept. 14, 1858, aged 57.

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ADDRESS

BY

J. W. DAWSON, LL. D.,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

ON SOME UNSOLVED PROBLEMS IN GEOLOGY.

LADIES AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

My predecessor in office remarked in the opening of his address that two courses are open to the retiring president of this Association in preparing the annual presidential discourse. He may either take up some topic relating to his own specialty, or he may deal with various or general matters relating to science and its progress. A geologist, however, is not necessarily tied up to one or the other alternative. His subject covers the whole history of the earth in time. At the beginning it allies itself with astronomy and physics and celestial chemistry. At the end it runs into human history and is mixed up with archæology and anthropology. Throughout its whole course it has to deal with questions of meteorology, geography and biology. In short, there is no department of physical or biological science, with which geology is not allied, or at least on which the geologist may not presume to trespass. When, therefore, I announce as my subject on the present occasion some of the unsolved problems of this universal science, you need not be surprised if I should be somewhat discursive.

Perhaps I shall begin at the utmost limits of my subject by remarking that in matters of natural and physical science, we are met at the outset with the scarcely solved question as to our own place in the nature which we study, and the bearing of this on the

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difficulties we encounter. The organism of man is decidedly a part of nature. We place ourselves, in this aspect, in the sub-kingdom vertebrata and class mammalia, and recognize the fact that man is the terminal link in a chain of being, extending throughout geological time. But the organism is not all of man, and when we regard man as a scientific animal, we raise a new question. If the human mind is a part of nature then it is subject to natural law, and nature includes mind as well as matter. On the other hand, without being absolute idealists we may hold that mind is more potent than matter, and nearer to the real essence of things. Our science is in any case necessarily dualistic, being the product of the reaction of mind on nature, and must be largely subjective and anthropomorphic. Hence, no doubt, arise much of the controversy of science and much of the unsolved difficulty. We recognize this when we divide science into that which is experimental or depends on apparatus, and that which is observational and classificatory — distinctions these which relate not so much to the objects of science as to our methods of pursuing them. This view also opens up to us the thought that the domain of science is practically boundless, for who can set limits to the action of mind on the universe or of the universe on mind. It follows that science must be limited on all sides by unsolved mysteries; and it will not serve any good purpose to meet these with clever guesses. If we so treat the enigmas of the sphinx nature, we shall surely be devoured. Nor, on the other hand, must we collapse into absolute despair and resign ourselves to the confession of inevitable ignorance. It becomes us rather boldly to confront the unsolved questions of nature, and to wrestle with their difficulties till we master such as we can, and cheerfully leave those we cannot overcome to be grappled with by our successors.

Fortunately, as a geologist, I do not need to invite your attention to those transcendental questions which relate to the ultimate constitution of matter, the nature of the ethereal medium filling space, the absolute difference or identity of chemical elements, the cause of gravitation, the conservation and dissipation of energy, the nature of life, or the primary origin of bioplasmic matter. I may take the much more humble role of an inquirer into the unsolved or partially solved problems which meet us in considering that short and imperfect record which geology studies in the rocky layers of the earth's crust, and which leads no farther back

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than to the time when a solid rind had already formed on the earth and was already covered with an ocean. This record of geology covers but a small part of the history of the earth and of the system to which it belongs, nor does it enter at all into the more recondite problems involved; still it forms, I believe, some necessary preparation at least to the comprehension of these.

What do we know of the oldest and most primitive rocks? At this moment the question may be answered in many and discordant ways; yet the leading elements of the answer may be given very simply. The oldest rock formation known to geologists is the Lower Laurentian, the fundamental gneiss, the Lewisian formation of Scotland, the Ottawa gneiss of Canada. This formation, of enormous thickness, corresponds to what the older geologists called the fundamental granite, a name not to be scouted, for gneiss is only a stratified granite. Perhaps the main fact in relation to this old rock is that it is a gneiss, that is, a rock at once bedded and crystalline, and having for its dominant ingredient the mineral orthoclase, a compound of silica, alumina and potash, in which are embedded, as in a paste, grains and crystals of quartz and hornblende. We know very well from its texture and composition that it cannot be a product of mere heat, and being a bedded rock we infer that it was laid down layer by layer in the manner of aqueous deposits. On the other hand, its chemical composition is quite different from that of the muds, sands and gravels usually deposited from water. Their special characters are caused by the fact that they have resulted from the slow decay of rocks like these gneisses, under the operation of carbonic acid and water, whereby the alkaline matter and the more soluble part of the silica have been washed away, leaving a residue mainly silicious and aluminous. Such more modern rocks tell of dry land subjected to atmospheric decay and rain-wash. If they have any direct relation to the old gneisses they are their grandchildren, not their parents. On the contrary, the oldest gneisses show no pebbles or sand or limestone — nothing to indicate that there was then any land undergoing atmospheric waste, or shores with sand and gravel. For all that we know to the contrary, these old gneisses may have been deposited in a shoreless sea, holding in solution or suspension merely what it could derive from a submerged crust recently cooled from a state of fusion, still thin, and exuding here and there through its fissures heated waters and volcanic products.

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It is scarcely necessary to say that I have no confidence in the supposition of unlike composition of the earth's mass on different sides on which Dana has partly based his theory of the origin of continents. The most probable conception seems to be that of Lyell, namely, a molten mass, uniform except in so far as denser material might exist toward its centre, and a crust at first approximately even and homogeneous, and subsequently thrown into great bendings upward and downward. This question has recently been ably discussed by Mr. Crosby in the London Geological Magazine.[1]

In short, the fundamental gneiss of the Lower Laurentian may have been the first rock ever formed; and in any case it is a rock formed under conditions which have not since recurred except locally. It constitutes the first and best example of those chemico-physical, aqueous or aqueo-igneous rocks, so characteristic of the earliest period of the earth's history. Viewed in this way the Lower Laurentian gneiss is probably the oldest kind of rock we shall ever know — the limit to our backward progress, beyond which there remains nothing to the geologist except physical hypotheses respecting a cooling incandescent globe. For the chemical conditions of these primitive rocks, and what is known as to their probable origin, I must refer you to my friend Dr. Sterry Hunt, to whom we owe so much of what is known of the older crystalline rocks[2] as well as of their literature, and the questions which they raise. My purpose here is to sketch the remarkable difference which we meet as we ascend into the Middle and Upper Laurentian.

In the next succeeding formation, the true Lower Laurentian of Logan, the Grenville series of Canada, we meet with a great and significant change. It is true we have still a predominance of gneisses which may have been formed in the same manner with those below them; but we find these now associated with great beds of limestone and dolomite, which must have been formed by the separation of calcium and magnesium carbonates from the sea water, either by chemical precipitation or by the agency of living beings. We have also quartzite, quartzose gneisses, and even pebble beds, which inform us of sand banks and shores. Nay, more, we have beds containing graphite which must be the residue of plants, and iron ores which tell of the deoxidation of iron

[1] June, 1883. [2] Hunt, Essays on Chemical Geology.

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oxide by organic matters. In short, here we have evidence of new factors in world-building, of land and ocean, of atmospheric decay of rocks, of deoxidizing processes carried on by vegetable life on the land and in the waters, of limestone-building in the sea. To afford material for such rocks, the old Ottawa gneiss must have been lifted up into continents and mountain masses. Under the slow but sure action of the carbonic dioxide dissolved in rain-water, its felspar had crumbled down in the course of ages. Its potash, soda, lime, magnesia and part of its silica had been washed into the sea, there to enter into new combinations and to form new deposits. The crumbling residue of fine clay and sand had been also washed down into the borders of the ocean, and had been there deposited in beds[3]. Thus the earth had entered into a new phase, which continues onward through the geological ages; and I place in your hands one key for unlocking the mystery of the world when I affirm that this great change took place, this new era of modern causes was inaugurated in the midst of the Laurentian period.

Was not this time a fit period for the first appearance of life? Should we not expect it to appear, independently of the evidence we have of the fact? I do not propose to enter here into that evidence, more especially in the case of the one well characterized Laurentian fossil, *Eozoon Canadense*. I have already amply illustrated it elsewhere. I would merely say here that we should bear in mind that in this later half of the Lower Laurentian, or if we so choose to style it, Middle Laurentian period, we have the conditions required for life in the sea and on the land; and since in other periods we know that life was always present when its conditions were present, it is not unreasonable to look for the first traces of life in this formation, in which we find for the first time the completion of those physical arrangements which make life, in such forms of it as exist on our planet, possible.

This is also a proper place to say something of the doctrine of what is termed metamorphism. The Laurentian rocks are undoubtedly greatly changed from their original state, more especially in the matters of crystallization and the formation of disseminated minerals, by the action of heat and heated water. Sandstones have thus passed into quartzites, clays into slates and schists,

[3] Dr. Hunt has now in preparation for the press an important paper on this subject, read before the National Academy of Sciences.

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limestones into marbles. So far, metamorphism is not a doubtful question; but when theories of metamorphism go so far as to suppose an actual change of one element for another, they go beyond the bounds of chemical credibility; yet such theories of metamorphism are often boldly advanced and made the basis of important conclusions. Dr. Hunt has happily given the name "metasomatosis" to this imaginary and impossible kind of metamorphism, which may be regarded as an extreme kind of evolution, akin to some of those forms of that theory employed with reference to life, but more easily detected and exposed. I would have it to be understood that, in speaking of the metamorphism of the older crystalline rocks, it is not to this metasomatosis that I refer, and that I hold that rocks which have been produced out of the materials decomposed by atmospheric erosion can never by any process of metamorphism be restored to the precise condition of the Laurentian rocks. Thus there is in the older formations a genealogy of rocks, which, in the absence of fossils, may be used with some confidence, but which does not apply to the more modern deposits. Still nothing in geology absolutely perishes, or is altogether discontinued; and it is probable that, down to the present day, the causes which produced the old Laurentian gneiss may still operate in limited localities. Then, however, they were general not exceptional. It is further to be observed that the term gneiss is sometimes of wide and even loose application. Beside the typical orthoclase and hornblendic gneiss of the Laurentian, there are micaceous, quartzose, garnetiferous and many other kinds of gneiss; and even gneissose rocks, which hold labradorite or anorthite instead of orthoclase, are sometimes, though not accurately, included in the term.

The Grenville series, or Middle Laurentian is succeeded by what Logan in Canada called the Upper Laurentian, and which other geologists have called the Norite or Norian series. Here we still have our old friends the gneisses, but somewhat peculiar in type, and associated with them are great beds, rich in lime-felspar, the so-called labradorite and anorthite rocks. The precise origin of these is uncertain, but this much seems clear, namely, that they originated in circumstances in which the great limestones deposited in the Lower or Middle Laurentian were beginning to be employed in the manufacture, probably by aqueo-igneous agencies, of lime-felspars. This proves the Norian rocks to be much

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younger than the Laurentian, and that, as Logan supposed, considerable earth movements had occurred between the two, implying lapse of time.

Next we have the Huronian of Logan, a series much less crystalline and more fragmentary, and affording more evidence of land elevation and atmospheric and aqueous erosion than any of the others. It has great conglomerates, some of them made up of rounded pebbles of Laurentian rocks, and others of quartz pebbles, which must have been the remains of rocks subjected to very perfect erosion. The pure quartz-rocks tell the same tale, while limestones and slates speak also of chemical separation of the materials of older rocks. The Huronian evidently implies movements in the previous Laurentian, and changes in its texture so great, that the former may be regarded as a comparatively modern rock, though vastly older than any part of the Palæozoic series.

Still later than the Huronian, is the great Micaceous series, called by Hunt the Mont Alban or White mountain group, and the Taconian or Lower Taconic of Emmons, which recalls in some measure the conditions of the Huronian. The precise relations of these to the later formations and to certain doubtful deposits around Lake Superior, can scarcely be said to be settled, though it would seem that they are all older than the fossiliferous Cambrian rocks, which practically constitute the base of the Palæozoic. I have, I may say, satisfied myself, in regions which I have studied, of the existence and order of these rocks as successive formations, though I would not dogmatize as to the precise relations of those last mentioned, or as to the precise age of some disputed formations which may either be of the age of the older Eozoic formations or may be peculiar kinds of Palæozoic rocks modified by metamorphism. Probably neither of the extreme views now agitated is absolutely correct.

After what has been said, you will perhaps not be astonished that a great geological battle rages over the old crystalline rocks. By some geologists they are almost entirely explained away or referred to igneous action or to the alteration of ordinary sediments. Under the treatment of another school, they grow to great series of Pre-Cambrian rocks, constituting vast systems of formations, distinguishable from each other, not by fossils, but by differences of mineral character. I have already indicated the

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manner in which I believe the dispute will ultimately be settled, and the President of the Geological Section will treat it more fully in his opening address.

After the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves, in the Cambrian, in the presence of a nearly complete invertebrate fauna of protozoa, polyps, echinoderms, mollusks and crustacea, and this not confined to one locality merely, but apparently extended simultaneously throughout the ocean. This sudden incoming of animal life, along with the subsequent introduction of successive groups of invertebrates, and finally of vertebrate animals, furnishes one of the greatest of the unsolved problems of geology, which geologists were wont to settle by the supposition of successive creations. In an address delivered at the Detroit meeting of the Association in 1875, I endeavored to set forth the facts as to this succession, and the general principles involved in it, and to show the insufficiency of the theories of evolution suggested by biologists to give any substantial aid to the geologist in these questions. In looking again at the points there set forth, I find they have not been invalidated by subsequent discoveries, and that we are still nearly in the same position with respect to these great questions that we were in at that time,— a singular proof of the impotency of that deductive method of reasoning which has become fashionable among naturalists of late. Yet the discussions of recent years have thrown some additional light on these matters, and none more so than the mild disclaimers with which my friend Dr. Asa Gray and other moderate and scientific evolutionists have met the extreme views of such men as Romanes, Hæckel, Lubbock and Grant Allen. It may be useful to note some of these, as shedding a little light on this dark corner of our unsolved problems.

It has been urged on the side of rational evolution that this hypothesis does not profess to give an explanation of the absolute origin of life on our planet, or even of the original organization of a single cell or of a simple mass of protoplasm, living or dead. All experimental attempts to produce by synthesis the complex albuminous substances or to obtain the living from the non-living, have so far been fruitless, and indeed we cannot imagine any process by which such changes could be effected. That they have

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been effected we know, but the process employed by their maker is still as mysterious to us as it probably was to him who wrote the words:—"And God said let the waters swarm with swarmers." How vast is the gap in our knowledge and our practical power implied in this admission, which must however be made by every mind not absolutely blinded by a superstitious belief in those forms of words which too often pass current as philosophy.

But if we are content to start with a number of organisms ready made—a somewhat humiliating start however—we still have to ask —How do these vary so as to give new species? It is a singular illusion in this matter, of men who profess to be believers in natural law, that variation may be boundless, aimless and fortuitous, and that it is by spontaneous selection from varieties thus produced that development arises. But surely the supposition of mere chance and magic is unworthy of science. Varieties must have causes, and these causes and their effects must be regulated by some law or laws. Now it is easy to see that they cannot be caused by a mere innate tendency in the organism itself. Every organism is so nicely equilibrated that it has no such spontaneous tendency, except within the limits set by its growth and the law of its periodical changes. There may, however, be equilibrium more or less stable. I believe all attempts hitherto made have failed to account for the fixity of certain, nay of very many, types throughout geological time, but the mere consideration that one may be in a more stable state of equilibrium than another, so far explains it. A rocking stone has no more spontaneous tendency to move than an ordinary boulder, but it may be made to move with a touch. So it probably is with organisms. But if so, then the causes of variation are external, as in many cases we actually know them to be, and they must depend on instability or change in surroundings, and this so arranged as not to be too extreme in amount and to operate in some determinate direction. Observe how remarkable the unity of the adjustments involved in such a supposition! how superior they must be to our rude and always more or less unsuccessful attempts to produce and carry forward varieties and races in definite directions! This cannot be chance. If it exists it must depend on plans deeply laid in the nature of things, else it would be most monstrous magic and causeless miracle. Still more certain is this conclusion when we consider the vast

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and orderly succession made known to us by geology, and which must have been regulated by fixed laws, only a few of which are as yet known to us.

Beyond these general considerations, we have others of a more special character, based on palæontological facts, which show how imperfect are our attempts as yet to reach the true causes of the introduction of genera and species.

One is the remarkable fixity of the leading types of living beings in geological time. If, instead of framing, like Hæckel, fanciful phylogenies, we take the trouble with Barrande and Gaudry, to trace the forms of life through the period of their existence, each along its own line, we shall be greatly struck with this, and especially with the continuous existence of many low types of life through vicissitudes of physical conditions of the most stupendous character, and over a lapse of time scarcely conceivable. What is still more remarkable is that this holds in groups which, within certain limits, are perhaps the most variable of all. In the present world, no creatures are individually more variable than the protozoa; as, for example, the foraminifera and the sponges. Yet these groups are fundamentally the same, from the beginning of the Palæozoic until now, and modern species seem scarcely at all to differ from specimens procured from rocks at least half-way back to the beginning of our geological record. If we suppose that the present sponges and foraminifera are the descendants of those of the Silurian period, we can affirm that in all that vast lapse of time, they have on the whole made little greater change than that which may be observed in variable forms at present. The same remark applies to other low animal forms. In forms somewhat higher and less variable, this is equally noteworthy. The pattern of the venation of the wings of cockroaches, and the structure and form of land snails, gally-worms and decapod crustaceans were all settled in the Carboniferous age, in a way that still remains. So were the foliage and the fructification of club-mosses and ferns. If, at any time, members of these groups branched off, so as to lay the foundation of new species, this must have been a very rare and exceptional occurrence and one demanding even some suspension of the ordinary laws of nature.

Certain recent utterances of eminent scientific men in England

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and France, are most instructive with reference to the difficulties which encompass this subject. Huxley, at present the leader of English evolutionists, in his "Rede Lecture"[4] delivered at Cambridge, England, holds that there are only two "possible alternative hypotheses" as to the origin of species—(1) that of "construction" or the mechanical putting together of the materials and parts of each new species separately; and (2) that of "evolution," or that one form of life "proceeded from another" by the "establishment of small successive differences." After comparing these modes, much to the disadvantage of the first, he concludes with the statement that "this was his case for evolution, which he rested wholly on arguments of the kind he had adduced"—these arguments being the threadbare false analogy of ordinary reproduction and the transformation of species, and the mere succession of forms more or less similar in geological time, neither of them having any bearing whatever on the origin of any species or on the cause of the observed succession. With reference to the two alternatives, while it is true that no certain evidence has yet been obtained either by experiment observation or sound induction as to the mode of origin of any species, enough is known to show that there are numerous possible methods, grouped usually under the heads of absolute creation, mediate creation, critical evolution and gradual evolution. It is also true that almost the only thing we certainly know in the matter is that the differences characteristic of classes, orders, genera and species must have arisen, not in one or two, but in many ways. An instructive commentary on the capacity of our age to deal with these great questions is afforded by the fact that this little piece of clever mental gymnastic should have been practised in a university lecture and in presence of an educated audience. It is also deserving of notice that though the lecturer takes the development of the *Nautili* and their allies as his principal illustration, he evidently attaches no weight to the argument in the opposite sense deduced by Barrande, the man of all others most profoundly acquainted with these animals, from the Palæozoic cephalopods.[5]

Another example is afforded by a lecture recently delivered at the Royal Institution in London by Prof. Flower.[6] The subject

[4] Report in "Nature," June 21, corrected by the author.
[5] Important facts bearing on this point have also been recently discovered by Hyatt.
[6] Reported in "Nature."

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is, "The Whales, past and present, and their probable origin." The latter point, as is well known, Gaudry had candidly given up. "We have questioned," he says, "these strange and gigantic sovereigns of the Tertiary oceans as to their ancestors,—they leave us without reply." Flower is bold enough to face this problem, and he does so in a fair and vigorous way, though limiting himself to the supposition of slow and gradual change. He gives up at once, as every anatomist must, the idea of an origin from fishes or reptiles. He thinks the ancestors of the whales must have been quadrupedal mammals. He is obliged for good reasons to reject the seals and the otters, and turns to the ungulates, though here also the difficulties are formidable. Finally, he has recourse to an imaginary ancestor, supposed to have haunted marshes and rivers of the Mesozoic age, and to have been intermediate between a hippopotamus and a dolphin, and omnivorous in diet. As this animal is altogether unknown to geology or zoology, and not much less difficult to account for than the whales themselves, he very properly adds: "Please to recollect, however, that this is a mere speculation." He trusts, however, that such speculations are "not without their use;" but this will depend upon whether or not they lead men's minds from the path of legitimate science into the quicksands of baseless conjecture.

Gaudry, in his recent work, "Enchainements du Monde Animal,"[7] though a strong advocate of evolution, is obliged in his final resumé to say: "Il ne laisse point percer le mystère qui entoure le developpement primitif des grandes classes du monde animal. Nul homme ne sait comment ont été formés les premiers individus de foraminifères, de polypes, d'étoiles de mer, de crinoides, etc. Les fossiles primaires ne nous ont pas encore fourni de preuves positives du passage des animaux d'une classe à ceux d'une autre classe."

Prof. Williamson, of Manchester, in an address delivered in February last before the Royal Institution of Great Britain, after showing that the conifers, ferns and lycopods of the Palæozoic have no known ancestry, uses the significant words: "The time has not yet arrived for the appointment of a botanical king-at-arms and constructor of pedigrees."

Another caution which a palæontologist has occasion to give with regard to theories of life, has reference to the tendency of

[7] Paris, 1883.

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biologists to infer that animals and plants were introduced under embryonic forms, and at first in few and imperfect species. Facts do not substantiate this. The first appearance of leading types of life is rarely embryonic. On the contrary, they often appear in highly perfect and specialized forms, often however of composite type and expressing characters afterwards so separated as to belong to higher groups. The trilobites of the Cambrian are some of them of few segments and so far embryonic, but the greater part are many-segmented and very complex. The batrachians of the Carboniferous present many characters higher than those of their modern successors and now appropriated to the true reptiles. The reptiles of the Permian and Trias usurped some of the prerogatives of the mammals. The ferns, lycopods and equisetums of the Devonian and Carboniferous were, to say the least, not inferior to their modern representatives. The shell-bearing cephalopods of the Palæozoic would seem to have possessed structures now special to a higher group, that of the cuttle-fishes. The bald and contemptuous negation of these facts by Hæckel and other biologists does not tend to give geologists much confidence in their dicta.

Again, we are now prepared to say that the struggle for existence, however plausible as a theory, when put before us in connection with the productiveness of animals and the few survivors of their multitudinous progeny, has not been the determining cause of the introduction of new species. The periods of rapid introduction of new forms of marine life were not periods of struggle but of expansion — those periods in which the submergence of continents afforded new and large space for their extension and comfortable subsistence. In like manner, it was continental emergence that afforded the opportunity for the introduction of land animals and plants. Further, in connection with this, it is now an established conclusion that the great aggressive faunas and floras of the continents have originated in the north, some of them within the arctic circle, and this in periods of exceptional warmth, when the perpetual summer sunshine of the arctic regions coëxisted with a warm temperature. The testimony of the rocks thus is that not struggle but expansion furnished the requisite conditions for new forms of life, and that the periods of struggle were characterized by depauperation and extinction.

But we are sometimes told that organisms are merely mechani-

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cal, and that the discussions respecting their origin have no significance any more than if they related to rocks or crystals, because they relate merely to the organism considered as a machine, and not to that which may be supposed to be more important, namely, the great determining power of mind and will. That this is a mere evasion by which we really gain nothing, will appear from a characteristic extract of an article by an eminent biologist in the new edition of the Encyclopedia Britannica, a publication which, I am sorry to say, instead of its proper role as a repertory of facts, has become a strong partisan, stating extreme and unproved speculations as if they were conclusions of science. The statement referred to is as follows: "A mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which or its vital phenomena depend on the one hand on its construction and on the other on the energy supplied to it; and to speak of vitality as anything but the name for a series of operations is as if one should talk of the horology of a clock." It would I think scarcely be possible to put into the same number of words a greater amount of unscientific assumption and unproved statement than in this sentence. Is "living protoplasm" different in any way from dead protoplasm, and if so, what causes the difference? What is a "machine?" Can we conceive of a self-produced or uncaused machine, or one not intended to work out some definite results? The results of the machine in question are said to be "vital phenomena;" certainly most wonderful results, and greater than those of any machine man has yet been able to construct. But why "vital?" If there is no such thing as life, surely they are merely physical results. Can mechanical causes produce other than physical effects? To Aristotle, life was "the cause of form in organisms." Is not this quite as likely to be true as the converse proposition? If the vital phenomena depend on the "construction" of the machine, and the "energy supplied to it," whence this construction and whence this energy? The illustration of the clock does not help us to answer this question. The construction of the clock depends on its maker, and its energy is derived from the hand that winds it up. If we can think of a clock which no one has made and which no one winds, a clock constructed by chance, set in harmony with the universe by chance, wound up periodically by chance, we shall then have an idea parallel to that of an organism living yet without any vital en-

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ergy or creative law, but in such a case we should certainly have to assume some antecedent cause, whether we call it "horology" or by some other name. Perhaps the term evolution would serve as well as any other, were it not that common sense teaches that nothing can be spontaneously evolved out of that in which it did not previously exist.

There is one other unsolved problem in the study of life by the geologist to which it is still necessary to advert. This is the inability of palæontology to fill up the gaps in the chain of being. In this respect we are constantly taunted with the imperfection of the record; but facts show that the record is much more complete than is generally supposed. Over long periods of time and many lines of being we have a nearly continuous chain, and if this does not show the tendency desired, the fault is as likely to be in the theory as in the record. On the other hand, the abrupt and simultaneous appearance of new types in many specific and generic forms and over wide and separate areas at one and the same time, is too often repeated to be accidental. Hence palæontologists in endeavoring to establish evolution, have been obliged to assume periods of exceptional activity in the introduction of species alternating with others of stagnation, a doctrine differing very little from that of special creation as held by the older geologists.

The attempt has lately been made to account for these breaks by the assumption that the geological record relates only to periods of submergence and gives no information as to those of elevation. This is manifestly untrue. In so far as marine life is concerned, the periods of submergence are those in which new forms abound for very obvious reasons already hinted; but the periods of new forms of land and fresh-water life are those of elevation, and these have their own records and monuments, often very rich and ample, as for example the swamps of the Carboniferous, the transition from the Cretaceous subsidence to the Laramie elevation, the Tertiary lake-basins of the west, the Terraces and raised beaches of the Pleistocene. Had I time to refer in detail to the breaks in the continuity of life which cannot be explained by the imperfection of the record, I could show at least that nature in this case does advance *per saltum* —by leaps, rather than by a slow continuous process. Many able reasoners, as LeConte in this country, and Mivart and Collard in England, hold this view.

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Here, as elsewhere, a vast amount of steady conscientious work is required to enable us to solve the problems of the history of life. But if so, the more the hope for the patient student and investigator. I know nothing more chilling to research or unfavorable to progress than the promulgation of a dogmatic decision that there is nothing to be learned but a merely fortuitous and uncaused succession, amenable to no law, and only to be covered, in order to hide its shapeless and uncertain proportions, by the mantle of bold and gratuitous hypothesis.

So soon as we find evidence of continents and oceans we raise the question, Have these continents existed from the first in their present position and form, or have the land and water changed places in the course of geological time? In reality both statements are true in a certain limited sense. On the one hand, any geological map whatever suffices to show that the general outline of the existing land began to be formed in the first and oldest crumplings of the crust. On the other hand, the greater part of the surface of the land consists of marine sediments which must have been derived from land that has perished in the process, while all the continental surfaces, except perhaps, some high peaks and ridges, have been many times submerged. Both of these apparently contradictory statements are true; and without assuming both it is impossible to explain the existing contours and reliefs of the surface.

In the case of North America, the form of the old nucleus of Laurentian rock in the north already marks out that of the finished continent, and the successive later formations have been laid upon the edges of this, like the successive loads of earth dumped over an embankment. But in order to give the great thickness of the Palæozoic sediments, the land must have been again and again submerged and for long periods of time. Thus, in one sense, the continents have been fixed; in another, they have been constantly fluctuating. Hall and Dana have well illustrated these points in so far as eastern North America is concerned. Prof. Hull of the Geological Survey of Ireland has recently had the boldness to reduce the fluctuations of land and water as evidenced in the British Islands to the form of a series of maps intended to show the physical geography of each successive period. The attempt is probably premature, and has been met with much adverse criticism; but there can be no doubt that it has an element of truth. When

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we attempt to calculate what could have been supplied from the old Eozoic nucleus by decay and aqueous erosion, and when we take into account the greater local thickness of sediments towards the present sea-basins, we can scarcely avoid the conclusion that extensive areas once occupied by high land are now under the sea. But to ascertain the precise areas and position of these perished lands may now be impossible.

In point of fact we are obliged to believe in the contemporaneous existence in all geological periods, except perhaps the very oldest, of three sorts of areas on the surface of the earth: (1) Oceanic areas of deep sea, which must always have occupied the bed of the present ocean or parts of it; (2) Continental plateaus sometimes existing as low flats or as higher table-lands and sometimes submerged; (3) Areas of plication or folding, more especially along the borders of the oceans, forming elevated lands rarely submerged and constantly affording the material of sedimentary accumulations.

Every geologist knows the contention which has been occasioned by the attempts to correlate the earlier Palæozoic deposits of the Atlantic margin of North America with those forming at the same time on the interior plateau, and with those of intervening lines of plication and igneous disturbance. Stratigraphy, lithology and fossils, are all more or less at fault in dealing with these questions, and while the general nature of the problem is understood by many geologists, its solution in particular cases is still a source of apparently endless debate.

The causes and mode of operation of the great movements of the earth's crust which have produced mountains, plains and table-lands, are still involved in some mystery. One patent cause is the unequal settling of the crust toward the centre; but it is not so generally understood as it should be, that the greater settlement of the ocean-bed has necessitated its pressure against the sides of the continents in the same manner that a huge ice-floe crushes a ship or a pier. The geological map of North America shows this at a glance, and impresses us with the fact that large portions of the earth's crust have not only been folded but bodily pushed back for great distances. On looking at the extreme north, we see that the great Laurentian mass of central Newfoundland has acted as a protecting pier to the space immediately west of it, and has caused the gulf of St. Lawrence to remain an un-

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disturbed area since Palæozoic times. Immediately to the south of this, Nova Scotia and New Brunswick are folded back. Still farther south, as Guyot has shown, the old sediments have been crushed in sharp folds against the Adirondack mass, which has sheltered the table-land of the Catskills and of the great lakes. South of this again the rocks of Pennsylvania and Maryland have been driven back in a great curve to the west. Nothing, I think, can more forcibly show the enormous pressure to which the edges of the continents have been exposed, and at the same time the great sinking of the ocean-beds. Complex and difficult to calculate though these movements of plication are, they are more intelligible than the apparently regular pulsations of the flat continental areas, whereby they have alternately been below and above the waters, and which must have depended on somewhat regularly recurring causes, connected either with the secular cooling of the earth or with the gradual retardation of its rotation or with both. Throughout these changes, each successive elevation exposed the rocks for long ages to the decomposing influence of the atmosphere. Each submergence swept away and deposited as sediment the material accumulated by decay. Every change of elevation was accompanied with changes of climate, and with modifications of the habitats of animals and plants. Were it possible to restore accurately the physical geography of the earth in all these respects, for each geological period, the data for the solution of many difficult questions would be furnished.

It is an unfortunate circumstance that conclusions in geology arrived at by the most careful observation and induction do not remain undisturbed, but require constant vigilance to prevent them from being overthrown. Sometimes, of course, this arises from new discoveries throwing new light on old facts; but when this occurs it rarely works the complete subversion of previously received views. The more usual case is that some over zealous specialist suddenly discovers what seems to him to overturn all previous beliefs, and rushes into print with a new and plausible theory which at once carries with him a host of half informed people, but the insufficiency of which is speedily made manifest.

Had I written this address a few years ago, I might have referred to the mode of formation of coal as one of the things most surely settled and understood. The labors of many eminent geologists, microscopists and chemists in the old and the new worlds

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had shown that coal nearly always rests upon old soil surfaces penetrated with roots, and that coal-beds have in their roofs erect trees, the remains of the last forests that grew upon them. Logan and the writer have illustrated this in the case of the series of more than sixty successive coal beds exposed at the South Joggins, and have shown unequivocal evidence of land surfaces at the time of the deposition of the coal. Microscopical examination has proved that these coals are composed of the materials of the same trees whose roots are found in the underclays, and their stems and leaves in the roof shales; that much of the material of the coal has been subjected to subaërial decay at the time of its accumulation; and that in this, ordinary coal differs from bituminous shale, earthy bitumen and some kinds of cannel, which have been formed under water; that the matter remaining as coal consists almost entirely of epidermal tissues, which being suberose in character are highly carbonaceous, very durable and impermeable by water,[8] and are, hence, the best fitted for the production of pure coal; and finally that the vegetation and the climatal and geographical features of the coal period were eminently fitted to produce in the vast swamps of that period, precisely the effects observed. All these points and many others have been thoroughly worked out for both European and American coal-fields, and seemed to leave no doubt on the subject. But several years ago certain microscopists observed on slices of coal layers filled with spore-cases, a not unusual circumstance, since these were shed in vast abundance by the trees of the coal forests, and because they contain suberose matter of the same character with epidermal tissues generally. Immediately we were informed that all coal consists of spores, and this being at once accepted by the unthinking, the results of the labors of many years are thrown aside in favor of this crude and partial theory. A little later, a German microscopist has thought proper to describe coal as made up of minute algæ, and tries to reconcile this view with the appearances, devising at the same time a new and formidable nomenclature of generic and specific names, which would seem largely to represent mere fragments of tissues. Still later, some local facts in a French coal-field have induced an eminent botanist of that country to revive the drift theory of coal, in opposition to that of growth *in situ*. A year or two ago, when my friend Professor Williamson of Manchester, informed me that

[8] Acadian Geology, third edition, supplement, p. 68.

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he was preparing a large series of slices of coal with the view of revising the whole subject, I was inclined to say that after what had been done by Lyell, Goeppert, Logan, Hunt, Newberry and myself, this was scarcely necessary; but in view of what I have just stated, it may be that all he can do will be required to rescue from total ruin the results of our labors even should he make no new discoveries.

An illustration of a different character is afforded by the controversy now raging with respect to the so-called fucoids of the ancient rocks. At one time the group of fucoids or algæ constituted a general place of refuge for all sorts of unintelligible forms and markings; graptolites, worm-trails, crustacean tracks, shrinkage-cracks, and above all rill-markings forming a heterogeneous group of fucoidal remains distinguished by generic and specific names. To these were also added some true land plants, badly preserved or exhibiting structures not well understood by botanists. Such a group was sure to be eventually dismembered. The writer has himself done something toward this,[9] but Professor Nathorst has done still more,[10] and now some intelligible explanation can be given of many of these forms. Quite recently, however, the Count de Saporta, in an elaborate illustrated memoir[11] has come to the defence of the fucoids, more especially against the destructive experiments of Nathorst, and would carry back into the vegetable kingdom many things which would seem to be mere trails of animals. While writing this address, I have received from Professor Crié of Rennes, a paper in which he not only supports the algal nature of *Rusichnites*, *Arthrichnites* and many other supposed fucoids, but claims for the vegetable kingdom even *Receptaculites* and *Archæocyathus*. It is not to be denied that some of the facts which he cites respecting the structure of the *Siphoniæ* and of certain modern encrusting *Algæ* are very suggestive, though I cannot agree with his conclusions. My own experience has convinced me that, while non-botanical geologists are prone to mistake all kinds of markings for plants, even good botanists, when not familiar with the chemical and mechanical conditions of fossilization, and with the present phenomena of tidal shores, are quite as easily misled, though they

[9] Footprints and Impressions on Carboniferous Rocks, Am. Jour. of Science, 1873.
[10] Royal Swedish Academy, Stockholm, 1881.
[11] Apropos des Algues fossiles, Paris, 1883.

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are very prone on the other hand to regard land plants of some complexity, when badly preserved, as mere algæ. In these circumstances it is very difficult to secure any consensus, and the truth is only to be found by careful observation of competent men. One trouble is that these usually obscure markings have been despised by the greater number of palæontologists, and probably would not now be so much in controversy were it not for the use made of them in illustrating supposed phylogenies of plants.

It would be wrong to close this address without some reference to that which is the veritable *pons asinorum* of the science, the great and much debated glacial period. I trust that you will not suppose that in the end of an hour's address, I am about to discuss this vexed question. Time would fail me even to name the hosts of recent authors who have contended in this arena. I can hope only to point out a few landmarks which may aid the geological adventurer in traversing the slippery and treacherous surface of the hypothetical ice-sheets of pleistocene times, and in avoiding the yawning crevasses by which they are traversed.

No conclusions of geology seem more certain than that great changes of climate have occurred in the course of geological time, and the evidence of this in that comparatively modern period which immediately preceded the human age is so striking that it has come to be known as preëminently the ice age; while in the preceding tertiary periods, temperate conditions seem to have prevailed even to the pole. Of the many theories as to these changes which have been proposed, two seem at present to divide the suffrages of geologists, either alone or combined with each other. These are (1)the theory of the precession of the equinoxes in connection with the varying eccentricity of the earth's orbit, advocated more especially by Croll; and (2) the different distribution of land and water as affecting the reception and radiation of heat and the ocean currents, a theory ably propounded by Lyell, and subsequently extensively adopted either alone or with the previous one. One of these views may be called the astronomical, the other the geographical. I confess that I am inclined to accept the second or Lyellian theory for such reasons as the following: (1) Great elevations and depressions of land have occurred in and since the Pleistocene, while the alleged astronomical changes are not certain, more especially in regard to their probable effect on the earth; (2) When the rival theories are tested by the present phenomena

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of the southern polar region and the North Atlantic, there seem to be geographical causes adequate to account for all except extreme and unproved glacial conditions; (3) The astronomical cause would suppose regularly recurring glacial periods of which there is no evidence, and it would give to the latest glacial age an antiquity which seems at variance with all other facts; (4) In those more northern regions where glacial phenomena are most pronounced, the theory of floating sheets of ice, with local glaciers descending to the sea, seems to meet all the conditions of the case, and these would be obtained, in the North Atlantic at least, by very moderate changes of level, causing, for example, the equatorial current to flow into the Pacific, instead of running northward as a gulf stream; (5) The geographical theory allows the supposition not merely of vicissitudes of climate quickly following each other in unison with the movements of the surface, but allows also of that near local approximation of regions wholly covered with ice and snow and others comparatively temperate which we see at present in the north.

If, however, we are to adopt the geographical theory, we must avoid extreme views, and this leads to the inquiry as to the evidence to be found for any such universal and extreme glaciation as is demanded by some geologists.

The only large continental area in the northern hemisphere supposed to be entirely ice- and snow-clad is Greenland, and this so far as it goes is certainly a local case, for the ice and snow of Greenland extend to the south as far as 60° N. latitude, while both in Norway and in the interior of North America the climate in that latitude permits the growth of cereals. Further, Grinnel Land, which is separated from North Greenland only by a narrow sound, has a comparatively mild climate, and as Nares has shown is covered with verdure in summer. Still further, Nordenskiold, one of the most experienced arctic explorers, holds that it is probable that the interior of Greenland is itself verdant in summer, and is at this moment preparing to attempt to reach this interior oasis. Nor is it difficult with the aid of the facts cited by Wœickoff and Whitney,[12] to perceive the cause of the exceptional condition of Greenland. To give ice and snow in large quantities, two conditions are required: first, atmospheric humidity, and, secondly, cold precipitating regions. Both of these conditions

[12] Memoir on Glaciers, Geol. Soc'y, Berlin, 1881. Climatic Changes, Boston, 1883.

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meet in Greenland. Its high coast ranges receive and condense the humidity from the sea on both sides of it and to the south. Hence the vast accumulation of its coast snow-fields, and the intense discharge of the glaciers emptying out of its valleys. When extreme glacialists point to Greenland and ask us to believe that in the glacial age the whole continent of North America as far south as the latitude of 40° was covered with a continental glacier, in some places several thousands of feet thick, we may well ask first what evidence there is that Greenland or even the Antarctic continent at present shows such a condition; and, secondly, whether there exists a possibility that the interior of a great continent could ever receive so large an amount of precipitation as that required. So far as present knowledge exists, it is certain that the meteorologist and the physicist must answer both questions in the negative. In short, perpetual snow and glaciers must be local and cannot be continental, because of the vast amount of evaporation and condensation required. These can only be possible where comparatively warm seas supply moisture to cold and elevated land, and this supply cannot in the nature of things penetrate far inland. The actual condition of interior Asia and interior America in the higher northern latitudes affords positive proof of this. In a state of partial submergence of our northern continents, we can readily imagine glaciation by the combined action of local glaciers and great ice-floes, but in whatever way the phenomena of the boulder clay and of the so-called terminal moraines are to be accounted for, the theory of a continuous continental glacier must be given up.

I cannot better indicate the general bearing of facts, as they present themselves to my mind in connection with this subject, than by referring to a paper by Dr. G. M. Dawson on the distribution of drift over the great Canadian plains, east of the Rocky mountains.[13] I am the more inclined to refer to this, because of its recency, and because I have so often repeated similar conclusions as to eastern Canada and the region of the Great Lakes.

The great interior plain of western Canada, between the Laurentian axis on the east and the Rocky mountains on the west, is seven hundred miles in breadth, and is covered with glacial drift presenting one of the greatest examples of this deposit in the world. Proceeding eastward from the base of the Rocky moun-

[13] Science, July 1, 1883.

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tains, the surface, at first more than 4,000 feet above the sea level, descends by successive steps to 690 feet, and is based on Cretaceous and Laramie rocks, covered by boulder clay and sand, in some places from one hundred to two hundred feet in depth, and filling up preëxisting hollows, though itself sometimes piled into ridges. Near the Rocky mountains the bottom of the drift consists of gravel not glaciated. This extends to about one hundred miles east of the mountains, and must have been swept by water out of their valleys. The boulder clay resting on this deposit is largely made up of local debris, in so far as its paste is concerned. It contains many glaciated boulders and stones from the Laurentian region to the east, and also smaller pebbles from the Rocky mountains, so that at the time of its formation there must have been driftage of large stones for seven hundred miles or more from the east, and of smaller stones from a less distance on the west. The former kind of material extends to the base of the mountains, and to a height of more than 4,000 feet. One boulder is mentioned as being $42 \times 40 \times 20$ feet in dimensions. The highest Laurentian boulders seen were at an elevation of 4,660 feet on the base of the Rocky mountains. The boulder clay when thick can be seen to be rudely stratified, and at one place includes beds of laminated clay with compressed peat, similar to the forest beds described by Worthen and Andrews in Illinois, and the so-called interglacial beds described by Hinde on Lake Ontario. The leaf beds on the Ottawa river and the drift trunks found in the boulder clay of Manitoba belong to the same category, and indicate that throughout the glacial period there were many forest oases far to the north. In the valleys of the Rocky mountains opening on these plains there are evidences of large local glaciers now extinct, and similar evidences exist on the Laurentian highlands on the east.

Perhaps the most remarkable feature of the region is that immense series of ridges of drift piled against an escarpment of Laramie and Cretaceous rocks, at an elevation of about 2,500 feet, and known as the "Missouri Coteau." It is in some places 30 miles broad and 180 feet in height above the plain at its foot, and extends north and south for a great distance; being in fact the northern extension of those great ridges of drift which have been traced south of the great lakes, and through Pennsylvania

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and New Jersey, and which figure on the geological maps as the edge of a continental glacier,—an explanation obviously inapplicable in those western regions where they attain their greatest development. It is plain that in the north it marks the western limit of the deep water of a glacial sea, which at some periods extended much farther west, perhaps with a greater proportionate depression in going westward, and on which heavy ice from the Laurentian districts on the east was wafted southwestward by the arctic currents, while lighter ice from the Rocky Mountains was being borne eastward from these mountains by the prevailing westerly winds. We thus have in the west on a very wide scale the same phenomena of varying submergence, cold currents, great ice floes and local glaciers producing icebergs, to which I have attributed the boulder clay and upper boulder drift of eastern Canada.

A few subsidiary points I may be pardoned for mentioning here. The rival theories of the glacial period are often characterized as those of land glaciation and sea-borne icebergs. But it must be remembered that those who reject the idea of a continental glacier hold to the existence of local glaciers on the high lands, more or less extensive during different portions of the great Pleistocene submergence. They also believe in the extension of these glaciers seawards, and partly water-borne in the manner so well explained by Mattieu Williams, in the existence of those vast floes and fields of current and tide-borne ice whose powers of transport and erosion we now know to be so great, and in a great submergence and reëlevation of the land bringing all parts of it and all elevations up to 5,000 feet successively under the influence of these various agencies, along with those of the ocean currents. They also hold that at the beginning of the glacial submergence, the land was deeply covered by decomposed rock, similar to that which still exists on the hills of the southern states, and which as Dr. Hunt has shown would afford not only earthy debris but large quantities of boulders ready for transportation by ice.

I would also remark that there has been the greatest possible exaggeration as to the erosive action of land ice. In 1865, after a visit to the Alpine glaciers, I maintained that in these mountains glaciers are relatively protective rather than erosive agencies, and that the detritus which the glacier streams deliver is derived mostly from the atmospherically wasted peaks and

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cliffs that project above them. Since that time many other observers have maintained like views, and very recently Mr. Davis of Cambridge and Mr. A. Irving have ably treated this subject.[14] Smoothing and striation of rocks are undoubtedly important effects both of land-glaciers and heavy sea-borne ice, but the levelling and filling agency of these is much greater than the erosive. As a matter of fact, as Newberry, Hunt, Belt, Spencer and others have shown, the glacial age has dammed up vast numbers of old channels which it has been left for modern streams partially to excavate.

The till or boulder clay has been called a "ground moraine," but there are really no Alpine moraines at all corresponding to it. On the other hand, it is more or less stratified, often rests on soft materials which glaciers would have swept away, sometimes contains marine shells, or passes into marine clays in its horizontal extension, and invariably in its embedded boulders and its paste, shows an unoxidized condition, which could not have existed if it had been a subaërial deposit. When the Canadian till is excavated and exposed to the air, it assumes a brown color, owing to oxidation of its iron, and many of its stones and boulders break up and disintegrate under the action of air and frost. These are unequivocal signs of a subaqueous deposit. Here and there we find associated with it, and especially near the bottom and at the top, indications of powerful water action, as if of land torrents acting at particular elevations of the land, or heavy surf and ice action on coasts, and the attempts to explain these by glacial streams have been far from successful. A singular objection sometimes raised against the subaqueous origin of the till is its general want of marine remains; but this is by no means universal, and it is well known that coarse conglomerates of all ages are generally destitute of fossils, except in their pebbles, and it is further to be observed that the conditions of an ice-laden sea are not those most favorable for the extension of marine life, and that the period of time covered by the glacial age must have been short compared with that represented by some of the older formations.

This last consideration suggests a question which might afford scope for another address of an hour's duration,—the question how long time has elapsed since the close of the glacial period. Recently the opinion has been gaining ground that the close of the

[14] Proceedings Boston Soc. Nat. Hist., XXII. Journal of Geological Society, February, 1883.

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ice-age is very recent. Such reasons as the following lead to this conclusion. The amount of atmospheric decay of rocks and of denudation in general which have occurred since the close of the glacial period are scarcely appreciable. Little erosion of river valleys or of coast terraces has occurred. The calculated recession of waterfalls and of production of lake ridges lead to the same conclusion. So do the recent state of bones and shells in the Pleistocene deposits and the perfectly modern facies of their fossils. On such evidence the cessation of the glacial cold and settlement of our continents at their present levels are events which may have occurred not more than 6000 or 7000 years ago, though such time estimates are proverbially uncertain in geology. This subject also carries with it the greatest of all geological problems, next to that of the origin of life, namely, the origin and early history of man. Such questions cannot be discussed in the closing sentences of an hour's address. I shall only draw from them one practical inference. Since the comparatively short Post-glacial and recent periods apparently include the whole of human history, we are but new comers on the earth and therefore have had little opportunity to solve the great problems which it presents to us. But this is not all. Geology as a science scarcely dates from a century ago. We have reason for surprise in these circumstances, that it has learned so much, but for equal surprise that so many persons appear to think it a complete and full-grown science and that it is entitled to speak with confidence on all the great mysteries of the earth that have been hidden from the generations before us. Such being the newness of man and of his science of the earth, it is not too much to say that humility, hard work in collecting facts, and abstinence from hasty generalization should characterize geologists, at least for a few generations to come.

In conclusion, science is light, and light is good; but it must be carried high, else it will fail to enlighten the world. Let us strive to raise it high enough to shine over every obstruction which casts any shadow on the true interests of humanity. Above all, let us hold up the light and not stand in it ourselves.

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PAPERS READ

IN

GENERAL SESSIONS.

A Classication of the Natural Sciences. By T. Sterry Hunt of Montreal, Can.

[ABSTRACT.]

To frame a rational classification of the natural sciences, and to define their mutual relations, has often been attempted. The present writer, in an essay read before the National Academy of Sciences in April, 1881, and published in the *L. E. & D. Philosophical Magazine*, for October, 1881, with the title of "The Domain of Physiology," suggested the basis of such a scheme, and he now, at the request of some of his readers, ventures, for the first time, to embody in a concise and tabulated form the views then and there enunciated, in the hope that other students may find it not unworthy of their notice.

The study of material nature constitutes what the older scholars correctly and comprehensively termed physics (the words physical and natural being synonymous) and presents itself in a twofold aspect: — first, as descriptive; and, second, as philosophical, — a distinction embodied in the terms Natural History and Natural Philosophy, or, more concisely, in the words Physiography and Physiology. The latter word has, from the time of Aristotle, been employed, in this general sense, to designate the philosophical study of nature, and will so be used in the present classification.

The world of nature is divided into the inorganic or mineralogical, and the organic or biological, kingdoms; the division of the latter into vegetable and animal being a subordinate one. The

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natural history, or physiography, of the inorganic kingdom, takes cognizance of the sensible characters of chemical species, and gives us descriptive and systematic mineralogy, which have hitherto been restricted to native species, but, in their wider sense, include all artificial species as well. The study of native mineral species, their aggregations, and their arrangement as constituents of our planet, is the object of geognosy and physical geography. The physiography of other worlds gives rise to descriptive astronomy.

The natural philosophy of the inorganic kingdom, or mineral physiology, is concerned, in the first place, with what is generally called dynamics or physics; including the phenomena of ordinary motion, sound, temperature, radiant energy, electricity, and magnetism. Dynamics, in the abstract, regards matter in general, without relation to species; chemism generates therefrom mineralogical or so-called chemical species, which, theoretically, may be supposed to be formed from a single elemental substance or *materia prima*, by the chemical process. Dynamics and chemistry build up our inorganic world, giving rise to geogeny, and, as applied to other worlds, to theoretical astronomy.

Proceeding now to the organic kingdom, its physiographical study leads us first to organography, and then to descriptive and systematic botany and zoölogy, two great subdivisions of natural history. Coming, next, to consider the physiological aspects of organic nature, we find, besides the dynamical and chemical activities manifested in the mineral, other and higher ones which characterize the organic kingdom. On this higher plane of existence, are found portions of matter which have become individualized, exhibit irritability, the power of growth by assimilation, and of reproduction, and moreover establish relations with the external world by the development of organs; all of which characters are foreign to the mineral kingdom. These new activities are often designated as vital; but since this word is generally made to include at the same time other manifestations, which are simply dynamical or chemical, I have elsewhere proposed for the activities characteristic of the organism the term biotics (*βιοτικός*, pertaining to life). The physiology of matter in the abstract is dynamical, that of mineral species is both dynamical and chemical, while that of organized forms is at once dynamical, chemical, and biotical. The study of the biotical activities of matter leads to organogeny and morphology, while the relations of organisms to one an-

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other, and to the inorganic kingdom, give us physiological botany and zoölogy. We thus arrive at a comprehensive and simple scheme of the natural sciences, which I have endeavored to set forth in the subjoined table.

Natural Sciences.	Inorganic Nature.	Organic Nature.
Descriptive.	Mineral Physiography.	Biophysiography.
—	—	—
General Physiography.	Descriptive and Systematic	Organography;
or	Mineralogy;	Descriptive and Systematic
Natural History.	Geognosy; Geography;	Botany and Zoology.
	Descriptive Astronomy.	
Philosophical.	Mineral Physiology.	Biophysiology.
—	—	—
General Physiology	*Dynamics* or *Physics*;	*Biotics.*
or	*Chemistry.*	Organogeny; Morphology;
Natural Philosophy.	Geogeny; Theoretical	Physiological
	Astronomy.	Botany and Zoology.

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The evidence for evolution in the history of the extinct Mammalia. By E. D. Cope, of Philadelphia, Pa.

The subject to which I wish to call your attention this morning requires neither preface nor apology, as it is one with the discussion of which you are perfectly familiar. My object in bringing it before the general session of the association is in view of the fact that you are all familiar with it in a general way, and that it probably interests the members of sections who do not pursue the special branch to which it refers, as well as those who do: also since it has been brought before us in various public addresses for many years, during the meetings of this association, I thought it might be well to be introduced at this meeting, in order that we might not omit to have all the sides of this interesting question presented.

The interests which are involved in it are large: they are chiefly, however, of a mental and metaphysical character; they do not refer so much to industrial and practical interests, nor do they involve questions of applied science. They involve, however, questions of opinion, questions of belief, questions which affect human happiness, I venture to say, even more than questions of applied science; certainly, which affect the happiness of the higher grades of men and women more than food or clothing, because they relate to the states of our mind, explaining as they do the reasons of our relations to our fellow-beings, and to all other things by which we are surrounded, and the general system of the forces by which we are controlled. So it has always appeared to me: hence I have selected the department of biology, and have taken a great interest in this aspect of it.

The doctrine of evolution, as taught by the biologists of to-day, has several stages or parts of its presentation. First, the foundation principle is this: that the species of animals and of plants, the species of organic beings, as well as the various natural divisions into which these organic beings fall, have not always been as we see them to-day, but they have been produced by a process of change which has progressed from age to age through the influence of natural laws; that, therefore, the species which now exist are the descendants of other species which have existed heretofore, by the ordinary processes of reproduction; and that all the various structures of organic beings, which make them what

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they are, and which compel them to act as they now act, are the result of gradual or sudden modifications and changes during the periods of geologic time. That is the first phase or aspect which meets the naturalist or biologist.

Another phase of the question relates to the origin of that life itself which is supposed to inhabit or possess organic beings. There is an hypothesis of evolution which derives this life from no-life, which derives vitality from non-vitality. That is another branch of the subject, to which I cannot devote much attention to-day.

There is still another department of the subject, which relates to the origin of mind, and which derives the mental organization of the higher animals, especially of man, from preëxistent types of mental organization. This gives us a genealogy of mind, a history of the production or creation of mind, as it is now presented in its more complex aspects as a function of the human brain. This aspect of the subject is, of course, interesting; and upon that I can touch with more confidence than upon the question of the origin of life.

Coming now to the question of the origin of structures, we have by this time accumulated a vast number of facts which have been collated by laborious and faithful workers, in many countries and during many years; so that we can speak with a good deal of confidence on this subject also. As to the phenomena which meet the student of zoölogy and botany at every turn, I would merely repeat, what every one knows,—and I beg pardon of my biological friends for telling them a few well-known truths, for there may be those present who are not in the biological section,— the phenomena which meet the student of biology come under two leading classes. The first is the remarkable fidelity of species in reproducing their like. "Like produces like," is the old theorem, and is true in a great many cases; just as coins are struck from the die, just as castings are turned out from a common mould. It is one of the most wonderful phenomena of nature, that such complex organisms, consisting of so many parts, should be repeated from age to age, and from generation to generation, with such surprising fidelity and precision. This fact is the first that strikes the student of these sciences. The first impression of the ordinary person would be, that these things must continue unchanged. When I began to study zoölogy and botany, I was surprised to find there was a science of which I had no conception, and that was this remarkable

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reproduction of types one after another in continued succession. After a man has had this idea thoroughly assimilated by his honest and conscientious studies, he will be again struck with another class of facts. He will find, not unfrequently, that this doctrine does not apply. He will find a series of facts which show that many individuals fail to coincide with their fellows precisely, the most remarkable variations and the most remarkable half-way attitudes and double-sided aspects occurring; and he will come to the conclusion, sooner or later, that like does not produce like in some animals with the same precision and fidelity with which it is accomplished in other animals. So that we have these two classes of facts,—the one relating to, and expressing, the law of heredity; the other, which expresses the law of variation. I should not like to say which class of facts is the most numerously presented to the student. In the present fauna we find many groups of species and varieties; but exactly how many species we have, how many genera and families we have, we cannot definitely state. The more precise and exact a person is in his definition and in his analysis, the more definite his science becomes, and the more precise and scientific his work. Biology is a science of analysis of forms. What the scales are to the chemist and the physicist, the rule and measure are to the biologist. It is a question of dimension, a question of length and breadth and thickness, a question of curves, a question of crooked shapes or simple shapes,—rarely simple shapes, mostly crooked, generally bilateral. It requires that one should have a mechanical eye, and should have also something of an artistic eye, to appreciate these forms, to measure them, and to be able to compare and weigh them.

Now, when we come to arrange our shapes and our measurements, we find, as I said before, a certain number of identities, and a certain number of variations. This question of variation is so common and so remarkable, that it becomes perfectly evident to the specialist in each department, that like does not at all times produce like. It is perfectly clear, and I will venture the assertion that nearly all the biologists in this room will bear me witness, that variability is practically unlimited in its range, and multiplied in the number of its examples. That is to say; species vary by adding or by failing to retain certain characteristics; and generic and other characters are found to appear or disappear in accordance with some law to be discussed farther on.

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I believe that this is the simplest mode of stating and explaining the law of variation: that some forms acquire something which their parents did not possess; and that those which acquire something additional have to pass through more numerous stages than their ancestors; and those which lose something pass through fewer stages than their ancestors; and these processes are expressed by the terms "acceleration" and "retardation."

Of course we are met with the opposite side of the case,—the law of heredity. We are told that the facts there are not accounted for by any law of evolution; that we cannot pass from one class of facts to the other class of facts; that the law of the one class is not that of the other. Here is a question of rational processes, of ordinary reason. If the rules of chemistry are true in America, I imagine they are true in Australia and Africa, although I have not been there to see. If the law of gravitation is effective here, I do not need to go to Australia or New Zealand to ascertain whether it be true there. So, if we find in a group of animals a law sufficient to account for their creation, it is not necessary to know that others of their relatives have gone through a similar process. I am willing to allow the ordinary practical law of induction, the practical law of inference, to carry me over these gaps, over these interruptions. And I state the case in this way, because it is just here that some people differ from me, and it is just here that I say the simple question of rationality comes in. I cannot believe that nature's laws are so dissimilar, so irregular, so inexact, that those which we can see and understand in one place are not true in another; I also believe that the question of geological likelihood is similar to the question of geographical likelihood. If a given process be true in one of the geological periods, it is true in another; if it be true in one part of the world, it is true in another; because I find interruptions in the series here, it does not follow that their need be interruptions clear through from age to age. The assumption is on the side of the man who asserts that transitions have not taken place between forms which are now distinct.

We are told that we find no sort of evidence of that transition in past geological periods; we are assured that such changes have not taken place; we are even assured that no such sign of such transition from one species to another has ever been observed,—a most astonishing assertion to make *to* a biologist, or *by* a biologist; and such persons have even the temerity to cite special

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cases, as between the wolf and the dog. Many of our domestic dogs are nothing but wolves, which have been modified by the hand of man to a very slight extent indeed. Many dogs, in fact nearly all dogs, are descendants of wild species of various countries, and are variously modified.

To take the question of the definition of species. Supposing we have several species well defined, say four or five. In the process of investigation we obtain a larger number of individuals, many of which betray characters which invalidate the definitions. It becomes necessary to unite the four or five species into one. And so, then, because our system requires that we shall have accurate definitions (the whole basis of the system is definitions; you know the very comprehension of the subject requires definitions), we throw them all together, because we cannot define all the various special forms as we did before, until we have but one species. And the critic of the view of evolution tells us, "I told you so! There is but one species, after all. There is no such thing as a connection between species: you never will find it." Now, how many discoveries of this kind will be necessary to convince the world that there are connections between species? How long are we to go on finding connecting links, and putting them together, as we have to do for the sake of the definition, and then be told that we have, nevertheless, no intermediate forms between species? The matter is too plain for further comment. We throw them together, simply because our definitions require it. If we knew all the known individuals which have lived, we should have no species, we should have no genera. That is all there is of it. It is simply a question of a universal accretion of material, and the collection of information. I do not believe that the well-defined groups will be found to run together, as we call it, in any one geological period, certainly in no one recent period. We recognize, however, in looking backwards that they converge to a wonderful extent: one group has diverged at one period, and another one has become diversified in a different period; and so each one has its history, some beginning farther back than others, some reaching far back beyond the very beginning of the time when fossils could be preserved. I call attention to this view because it is a very easy matter for us to use words for the purpose of confusing the mind; for, next to the power of language to express clear ideas, is its power of expressing no ideas at all. As we all know, we can *say* many things

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which we cannot *think*. It is a very easy thing to say twice two are equal to six, but it is impossible to think it.

I would cite what I mean by variations of species in one of its phases: I mention a genus of snakes, Ophibolus, which is found in the United States. If we take the species of this snake-genus as found in the northern states, we have a good many species well defined. If we go to the gulf states, and examine our material, we see we have certain other species well defined, and they are very nicely distinguished. If, now, we go to the Pacific coast, to Arizona and New Mexico, we shall find another set of species well defined indeed. If we take all these different types of our specimens of different localities together, our species, as the Germans say, all tumble together: definitions disappear, and we have to recognize, out of the preliminary list of thirteen or fourteen, only four or five. That is simply a case of the kind of fact with which every biologist is perfectly familiar.

When we come to the history of the extinct forms of life, it is perfectly true that we cannot observe the process of descent in actual operation, because, forsooth, fossils are necessarily dead. We cannot perceive any activities, because fossils have ceased to act. But, if this doctrine be true, we should get the series, if there be such a thing; and we do, as a matter of fact, find longer or shorter series of structures, series of organisms proceeding from one form into another form, which are exactly as they ought to be if this process of development by descent had taken place.

I am careful to say this; because it is literally true, as we all must admit, that species must fall into some kind of order or other. You could not collect bottles, you could not collect old shoes, but you could make some kind of a serial order of them. There are, no doubt, characters by which such and such shoes could be distinguished from other shoes, these bottles from other bottles. But if serial order does not prove evolution, as is too often assumed, we have in recent forms of life in zoölogy and botany, irrefragable proofs of the metamorphoses, and transformations, and changes of the species, in accordance with the doctrine with which we commenced.

We now come to the second chapter of our subject. With the assumption, as I take it already satisfactorily proven, of species having changed into others, in considering this matter of geological succession or biological succession, I bring you face to

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face with the nature and mode of the change; and hence we may get a glance perhaps at its laws.

I have on the board a sketch or table which represents the changes which took place in certain of the mammalia. I give you a summary of the kind of thing which we find in one of the branches of paleontology. I have here two figures, one representing a restoration, and the other an actual picture, of two extinct species that belong to the early eocene periods. One represents the ancestor of the horse line, Hyracotherium, which has four toes on his anterior feet, and three behind; and the other, a type of animal, Phenacodus, which is antecedent to all the horse series, the elephant series, the hog, the rhinoceros, and all the other series of hoofed animals. It has five toes on all the feet. Each presents us with the primitive position in which their series first come to our knowledge in the history of geological time.

I have also arranged here a series of some leading forms of the three principal epochs of the mesozoic times, and six of the leading ones of the tertiary time. I have added some dates to show you the time when the faunæ which are entombed in those beds were discovered, in the course of our studies; and you will easily see how unsafe it is to say that any given type of life has never existed, or even to assert that such and such a form is unknown; and it is still more unsafe, I think, to assert that any given form of life properly defined, or that a specific intermediate form of life, will not be found. I think it is much safer to assert that such and such intermediate forms will be found. I have frequently had the pleasure of realizing anticipations of this kind. I have asserted that certain types would be found, and they have been found. You will see that I attend to the matter of time because there have been a great many things discovered in the last ten or fifteen years in this department. With these forms I give the date of the discovery of the fauna in which they are embraced.

Here we have the White-River fauna discovered in 1856; then we skip a considerable period of time, and the next one was in 1869, when the cretaceous series was found. Six or seven cretaceous faunæ have been discovered. Then we have the Bridger fauna in 1870, the Wasatch fauna in 1874. Next we have, in 1877, the Equus beds and the fauna which they embrace, part of which was also found in 1878. The Permian fauna, which is one of the last, in 1879; and the latest, the Puerco, which gives the oldest and ances-

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tral types of the modern forms of mammalia, was only found in 1881. When I first commenced the study of this subject, about 1860, there were perhaps 250 species known. There are now somewhere near 2,000, and we are augmenting them all the time. I have found many myself: if they were distributed through the days of the year, I think in some years I should have had several every day. You see then that the accessions to knowledge which are constantly being made make it unsafe to indulge in any prophecies; as for instance, that, because such and such things have not been found, therefore they cannot be; for we find such and such things really have been and really are discovered.

The successive changes that we have in the mammalia have taken place in the limbs, feet, teeth, and brain, and the vertebral column. The parts which present us with the greatest numbers of variations are those in which many parts are concerned, as in the limbs and feet. In the lower eocene (Puerco), the toes were 5–5. In the Loup-Fork fauna, some possess toes but 1–1. Prior to this period no such reduction was known, although in the Loup-Fork fauna a very few species remained 5–5. Through this entire series we have transitions steady and constant, from 5–5, to 4-5, to 4–4, to 4–3, to 3–3, to 2–2, to 1–1. In the Puerco period there was not a single mammal of any kind which had a good ankle-joint; which had an ankle-joint constructed as ankle-joints ought to be, with tongue and groove. The model ankle-joint is a tongue-and-groove arrangement. In this period they were nearly all perfectly flat. As time passes on we get them more and more grooved, until in the Loup-Fork fauna and the White-River fauna they are nearly all grooved. In the sole of the foot, in the Puerco fauna, they are all flat; but in the Loup-Fork fauna the sole of the foot is in the air, and the toes only are applied to the ground, with the exception of the line of monkeys, in which the feet have not become erect on the toes, and the elephant, in which the feet are nearly flat also, and the line of bears, where they are also flat. As regards the angulation between the small bones of the palm and of the sole, there is not a single instance in which those bones are locked in the lower eocene, as they are in the later and latest tertiary. So also with the articulation of the toes with the foot.

When we come to the limbs, the species of the Puerco fauna have short legs. They have gradually lengthened out, and in the late periods they are nearly all relatively long.

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Formation.	No of toes.	Feet.	Astragalus.	Carpus and tarsus.	Ulno-radius.	Superior molars.	Zygapophyses.	Brain.
Miocene . .	1-1 2-2	Digitigrade. (Plantigrade.)	Grooved. (Flat.)	Interlocking. (Opposite)	Faceted.	4-tubercles, crested and cemented.	Doubly involute. Singly involute.	Hemispheres larger, convoluted.
Upper . . . . . (Loup Fork.)	3-3 4-4 (5-5)							
Middle . . . . . (John Day.)	2-2 3-3 4-4	Digitigrade.	Grooved.	Interlocking.	Faceted. Smooth.	4-tubercles, and crested.	Singly involute. Doubly involute.	Hemispheres larger, convoluted.
Lower . . . . . (White River.)	3-3 4-3 4-4	Digitigrade. Plantigrade.	Grooved.	Interlocking.	Smooth. Faceted.	4-tubercles, and crested.	? Singly involute.	Hemispheres small; and larger.
Eocene . .	3-3	(Digitigrade.)	Grooved.	Opposite.	Smooth.	4-tubercles.	Singly involute.	Hemispheres small.
Upper	4-3	Plantigrade.	(Flat.)	Interlocking.		3-tubercles, and crested.	Plane.	
(Bridger.) . .	4-5 5-5							
Middle	4-3	Plantigrade.	Flat.	Opposite.		4-tubercles.	Plane.	Hemispheres small; mesencephalon sometimes exposed.
(Wasatch.)	4-5 5-5	(Digitigrade.)	(Grooved)	Interlocking.	Smooth.	3-tubercles, a few crested.	Singly involute.	
Lower	5-5	Plantigrade.	Flat.	Opposite.	Smooth.	3-tubercles. (4-tubercles), none crested.	Plane.	Mesencephalon; exposed hemisphere small and smoother.

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Coming to the vertebræ as a part of the osseous system, I mention the zygapophyses, or antero-posteriorly directed processes, of which the posterior looks down, and the anterior looks up. They move on each other, as the vertebral column bends from side to side. In the lower forms of vertebrates they are always flat, and in the hoofed mammals of the Puerco period they are all flat. In the Wasatch period we get a single group in which the articulation, instead of being perfectly flat, becomes rounded; in the later periods we get them very much rounded; and, finally, in the latest forms, we get the double curve and the locking process in the vertebral column, which, as in the limb, secures the greatest strength with the greatest mobility.

In the first stages of the growth of the spinal column, it is a notochord, or a cylinder of cartilage or softer material. In later stages the bony deposit is made in its sheath until it is perfectly segmented. Now, all the Permian land-animals, reptiles, and batrachians, retain this notochord with the elements of osseous vertebræ, in a greater or less degree of completeness. There are some in South Africa, I believe, in which the ossification has come clear through the notochord; but they are few. In this characteristic the Permian appears almost, perhaps absolutely, peculiar as regards land-animals. There is something to be said as to the condition of the column from a mechanical standpoint, and it is this: that the chorda exists, with its osseous elements disposed about it; and in the batrachians equally related to salamanders and frogs, these osseous elements are arranged in the sheath or skin of the chorda; and they are in the form of regular concave segments, very much like such segments as you can take from the skin of an orange,—but parts of a cylinder, and having greater or less dimensions according to the group or species. Now, the point of divergence of these segments is on the side of the column. The contacts are placed on the side of the column where the segments separate,—the upper segments rising and the lower segments coming downwards. To the upper segments are attached the arches and their articulations, and the lower segments are like the segments of a cylinder. If you take a flexible cylinder, and cover it with a more or less inflexible skin or sheath, and bend that cylinder sidewise, you of course will find that the wrinkles or fractures of that part of the surface will take place along the line of the shortest curve, which is on the side; and, as a matter of fact, you have

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breaks of very much the character of the segments of the Permian batrachia. It may not be so symmetrical as in the actual animal, for organic growth is symmetrical so far as not interfered with; for, when we have two forces, the one of hereditary growth, and the other of change or alteration, and they contend, you will find in the organic being a quite symmetrical result. That is the universal rule. In the cylinder bending both ways, of course the shortest line of curve is right at the centre of the side of that cylinder, and the longest curve is of course at the summit and base, and the shortest curve will be the point of fracture. And that is exactly what I presume has happened in the case of the construction of the segments of the sheath of the vertebral column, by the lateral motion of the animal in swimming, and which has been the actual cause of the disposition of the osseous material in its form. I have gone beyond the state of the discussion in calling attention to one of the forces which have probably produced this kind of result. That is the state of the vertebral column of many of the vertebrata of the Permian period.

I go back to the mammalia, and call attention to the teeth. The ordinary tooth of the higher type of the mammalia, whether hoofed or not, with some exceptions, is complex with crests or cusps. In cutting the complex grinding surfaces we find they have been derived by the infolding of extensions of four original cusps or tubercles. They have been flattened, have been rendered oblique, have run together, have folded up, have become acute, have descended deeply or have lifted themselves, so that we have teeth of all sorts and kinds, sometimes very elegant, and oftentimes very effective in mechanism. In many primary ungulates, the primitive condition of four conical tubercles is found. In passing to older periods we find the mammalia of the Puerco period, which never have more than three principal tubercles, with the exception of three or four species. In the succeeding periods, however, they get the fourth tubercle on the posterior side. Finally, you get a complicated series of grinding or cutting apparatus, as the case may be.

Last, but not least, we take the series of the brain. No doubt the generalization is true, that the primitive forms of mammalia had small brains with smooth hemispheres; later ones had larger brains with complex hemispheres. In general the carnivora have retained a more simple form of brain, while herbivorous animals

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have retained a more complicated type of brain. The lowest forms of mammalia display the additional peculiarity of having the middle brain exposed; and the hemispheres or large lobes of the brain, which are supposed to be the seat of the mental phenomena, are so reduced in size at the back end that you see the middle brain distinctly, though it is smaller than in reptiles and fishes.

It is beyond the possibility of controversy, that these series have existed, that they have originated in simplicity, and have resulted in complication; and the further induction must be drawn, that the process of succession has been towards greater effectiveness of mechanical work. There are also cases of degradation, as in the growing deficiency in dentition in man. There is no doubt that a large number of people are now losing their wisdom-teeth in both jaws.

We are now brought to the question of the relations which mind bears to these principles. The question as to the nature of mind is not so complex as it might seem. There is a great deal of it, to be sure; but on examination it resolves itself into a few ultimate forms. An analysis reduces it to a few principal types or departments,—the departments of the intelligence and of emotions (with their primary simpler forms, likes and dislikes), and the will, if such there be. These three groups, proposed by Kant, are well known, and are adopted by many metaphysicians; and they stand the scrutiny of modern science as applied to both men and the lower animals. But the question of the material of the mind, the original raw stuff out of which mind was made, is one which is claiming attention now from biologists, as it always has done from physiologists proper, and physicians. This is sensibility, mere simple sensibility, unmodified sensibility, or consciousness. Sensibility, in connection with memory, is sufficient for the accomplishment of wonderful results. It is only necessary to impress the sensibility with the stimuli which this world affords, whether from the outside or the inside, to have the record made, and to have the record kept. Among wonderful things this is perhaps the most wonderful: that a given form of matter should be able to retain a record of events, a record which is made during a greater or less degree of sensibility; which is retained in a state of insensibility; and is finally returned to the sensibility by some curious process of adhesion, as the results of impresses which are found on the material tissue concerned.

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And these simple elements of mind are found in animals. No zoologist who has perception or honesty, nor any farmer or breeder, nor any person who has charge of animals in any way, can deny sensibility to all the lower animals at times. The great stumbling-block in the way of the thinker in all this field, is the great evanescence of this sensibility; the great ease with which we dissipate it. The readiness with which we can deprive a fellow-being of his sense is a stumbling-block in more ways than one. While it is a question of the greatest difficulty, nevertheless, like other departments of nature, doubtless it will ultimately be explained by the researches of physiologists. I only need to call attention to consciousness as an important factor in evolution.

We now approach the question of the origin of organic machines with fresh resources. Did the consciousness of the animal find his structure made ready to hand, or did he, under the dire stimuli of necessity, produce through ages these modifications in his own structure? We are told by some of our friends, that law implies a lawgiver, that evolution implies an evolver: the next question is, Where is the lawgiver? where is the evolver? where are they located? This question is best answered, as it appears to me, as follows. In the first place, I may say, it is distinctly proven in some directions, that the constant applications of force or motion in the form of strains, in the form of impacts and blows, upon any given part of the animal organism, do not fail to produce results in change of structure. I believe the changes in the ungulates, to which I have called your attention, are the result of strains and impacts, precisely as I have shown you the manner of the fracture of the vertebral column of the primitive vertebrates of the Permian period. This would require long discussion to render clear; nevertheless, I venture to make the assertion that this series of structures is the result of definite and distinct organic forces, directed to special ends. We have yet to get at the conflicting forces which have produced the results we see. Mechanical evolution will give us a good deal to do for some time to come. Of course, if motion has had an effect in modifying structure, it behooves us to investigate those conditions which give origin to motion in animals. First in order come the sensibilities of the animal, which we have traced to simple consciousness, and stimuli, upon notice of which he immediately begins to move. The primary stimulus of all kinds of motion is necessarily touch. If a

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stone falls upon the tail of some animal which has a tail, he immediately gets out of that vicinity. If a jelly-fish with a stinging apparatus runs across an eel which has no scales, the eel promptly moves. External applications of unpleasant bodies will always cause an animal to change his location. Then he is constantly assaulted by the dire enemy of beasts, hunger, an instinct which is evidently universal, to judge from the actions of animals. This seems to have fashioned, in large part, all forms of life, from the least to the greatest, from the most unorganized to the most complex. Each exercised itself for the purpose of filling its stomach with protoplasm. Then come the stimuli which should be included under the class of touch, changes of temperature. No animals like to be cold or too hot; and when the temperature is disagreeable, the tendency is to go away from that locality. Among primary instincts must be included that of reproduction. After that comes the sensation of resistance, or, carried to a high degree, of anger: when an animal's interests are interfered with, its movements restricted, the most energetic displays are prompted. So, you see, it is a matter of necessity that mental phenomena lie at the back of evolution, provided always that the connecting link of the argument — that motion has ever affected structure — be true. That is a point which, of course, admits of much discussion. I have placed myself on the affirmative side of that question; and, if I live long enough, I expect to see it absolutely demonstrated.

Of course the development of mind becomes possible under such circumstances. It is not like a man lifting himself up by his boots, which it would be were there no such thing as memory. But with that memory which accumulates, which formulates, first habits, and then structures, especially in the soft, delicate nervous tissue, the development of the function of the mind, as well as the machinery of the mind becomes perfectly possible. We develop our intellect through the accumulation of exact facts; through the collation of pure truth, no matter whether it be a humble kind of truth,— as the knowledge of the changes of the seasons, which induces some animals to lay up the winter's store; whether it be knowledge of the fact that the sting of the bee is very unpleasant; or knowledge of the fact (of which the wild ox, no doubt, is thoroughly aware) that the teeth of the wolf are not pleasant to come in contact with; or whether it be the complex knowledge of man.

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When the cerebral matter has become larger and more complex, it receives and retains a much greater number of impressions, and the animal becomes a more highly educated being.

As regards the department of emotions or passions, they are also much stimulated by the environment. Animals which live in a state of constant strife naturally have their antagonistic passions much developed; while amiable, sympathetic sentiments are better and more largely produced by peace-loving animals. Thus it is that the various departments of the mind have the beautiful results which we now find in the human species.

There are some departments of the mind which some of our friends decline to admit having had such an origin. The moral faculty, for instance, is excepted by many from this series. But the reasons why they object to its production in this way are, to my mind, not valid. The development of the moral faculty, which is essentially the sense of justice, appears to them not to fall within the scope of a theory of descent or of evolution. It consists of two parts. First is the sentiment of benevolence, or of sympathy with mankind, which gives us the desire to treat them as they should be treated. It is not sufficient for justice that it is unmixed mercy or benevolence, which is sometimes very injurious, and very often misplaced. It requires, in the second place, the criticism of the judgment, of the mature intellect, of the rational faculty, to enable the possessor to dispose of his sentiments in the proper manner. The combination of rational discrimination and judgment, with benevolence, constitutes the sense of justice, which has been derived, no doubt, as a summary of the development of those two departments of the mind,—the emotions and the intellect.

It is said, that a sense of justice could not be derived from the sense of no justice; that it could not have been derived from the state of things which we find in the animals, because no animal is known to exhibit real justice, and that objection is valid as far as it goes. I suspect that no animal has been observed to show a true sense of justice. That they show sympathy and kindness, there is no question; but when it comes to real justice, they do not display it. But do all men display justice? Do all men *understand* justice? I am very sure not. There are a good many men in civilized communities, and there are many tribes, who do not know what justice is. It does not exist as a part of

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every mental constitution. I never lived among the Bushmen, and do not know exactly what their mental constitution is; but in a general way the justice of savages is restricted to the very smallest possible circle,—that of their tribe or of their own family. There is a class of people who do not understand justice. I do not refer to people who know what right is, and do not do it; but to the primitive state of moral character, in which, as in children, a sense of justice is unknown. I call attention to the fact, because some of our friends have been very much afraid that the demonstration of the law of evolution, physical and metaphysical, would result in danger to society. I suspect not. The mode in which I understand this question appears to me to be beneficial to society, rather than injurious; and I therefore take the liberty of appending this part of the subject to its more material aspect.

I refer to another topic, that is to the nature of life, and the physical basis of life. The word "life" is so complex that it is necessary to define it, and so to define it away that really the word "life" does not retain its usual definition. Many phenomena of life are chemical, physical, mechanical. We have to remove all these from consideration, because they come within the ordinary laws of mechanical forces; but we have a few things left which are of a different character. One is the law of growth, which is displayed in the processes of embryonic succession; secondly, the wonderful phenomena of sensibility. Those two things we have not yet reduced to any identity with the ordinary laws of force, though we know of their dynamic equivalency. In the phenomena of embryology the phenomena of evolution are repeated, only concentrated in the early stages through which animals have to pass. So whatever explains the general phenomena of evolution explains the phenomena of embryology.

What is the nature of physical sensibility? In this planet, it is found residing only in one form of matter, which has a slightly varied chemical constitution, namely, protoplasm, so called from a physical standpoint. Now, this world, as you all know, has passed through many changes of temperature. Its early periods, it is probable, were so very hot that protoplasm had a very poor chance. Again, can we assume for a moment that this little speck in the great universe is the only seat of life? I suppose scarcely any scientific man will venture do do so. If, therefore, life exists in other planets, worlds and systems, does it necessarily

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occupy bodies of protoplasm in those different, remote spheres? It would be a great assumption. It is altogether improbable. The certainty is, that in those planets which are in proximity to the sun's heat there could be no protoplasm. Protoplasm in the remote planets would be a hard mineral, and near the sun it would be dissipated into its component gases. So that, if life be found in other parts of this universe, it must reside in some different kind of material. It is extremely probable that the physical conditions that reside in protoplasm might be found in other kinds of matter. It is in its chemical inertness, and in its physical constitution, that its adaptation to life resides; and the physical constitution necessary for the sustenation of life may be well supposed to exist in matter in other parts of the universe. I only say the door is open and not closed: any one who asserts that life cannot exist in any other material basis than protoplasm is assuming more than the world of science will permit him to assume. And that it is confined to this single planet, among the great systems of the universe,—that assumption will not for a moment be allowed. Therefore the subject is one which allows us a free field for future investigation: it is by no means closed in the most important laws which it presents to the rational thinker. I hope, therefore, that, if the evidence in favor of this hypothesis of the creation of living forms be regarded as true, that no one will find in it any ground for any very serious modification of existing ideas on the great questions of right and wrong, which have long since been known by men as a result of experience, and without any other scientific demonstration whatsoever.

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SECTION A.

MATHEMATICS AND ASTRONOMY.

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ADDRESS

BY

WILLIAM A. ROGERS,

VICE PRESIDENT, SECTION A.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS.

The illustrious Argelander was accustomed to say, in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the Astronomische Gesellschaft, at its formation in 1865, was the proposition to determine accurately the coördinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organization) belongs the credit of arranging a scheme of observations by which, through the coöperation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labor were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definite results which the discussions of all the observations are expected to show, we may with profit consider the object sought in the undertaking, the gen-

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eral plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty; a problem of which what we call universal gravitation is only one element, if, indeed, it be an element; a problem which reaches farther than all others into the mysteries of the universe, — the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the Gesellschaft in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar coördinates as a base, but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1818 were referred to the origin from which stellar coördinates are reckoned, by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose coördinates were supposed to be well known. The unequalled Pulkowa observations for the epoch 1845 form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed; viz., Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, D'Agelet for 1783, Piazzi for 1805, Auwers' Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1824, Bessel for 1827, Argelander for 1830, and Pulkowa for 1845.

An analysis of the important catalogues of secondary stars published previous to 1865 reveals four important facts:—

1. That nearly all of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

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2. That in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

3. That with the exception of the polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

4. That each catalogue involves a system of errors peculiar to the observers, to the character of the instrument employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension, *inter se*, were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the Monthly Notices of the Royal Astronomical Society in 1861 (xxi, 245), "On the Positions of the Radcliffe catalogue." I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time in the treatment of this problem. It is as follows:—

Diff. of R. A. (Greenw. 12 Year Cat. — Rad.) $= -.038^{s} + .032^{s} \sin (a + 5^{h}\,32^{m})$. Extending this expression to terms of the second order, it may be put under the form, $\Delta =$ a constant $+ (m \sin a + n \cos a) + (m' \sin 2a + n' \cos 2a)$.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all is the more remarkable, since the primary stars upon which the Radcliffe positions depend, are nearly the same as

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those employed at Greenwich. In reality, the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference, but the existence of periodic errors in the Greenwich catalogue was not, however, overlooked by him. The speaker has shown (*Proc. Amer. Acad.*, 1874, 182) that the weight of the errors of the provisional catalogue assumed fell between the first and third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors either in one or in both, it is clearly impossible either to determine the nature of the errors, or to assign their true cause, until a fundamental system has been established which is free both from accidental and from periodic errors, — from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one, when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar coördinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits $-30°$ and $+46°$ declination. A comparison of the places of these stars for a given epoch, with the same stars in any catalogue for the same epoch, enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independ-

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ently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons, from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed: —

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Äbo catalogue for 1830, and the Pulkowa catalogue for 1845. One is reminded, in this connection, of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula, how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal Society, I have endeavored to show, that, with us, the experiment *entirely* fails."

b. During this interval, the constant differences between the earlier catalogues and Newcomb's system vary between $+ 0.17^s$ for Pond, 1820; and $- 0.19^s$ for Pond, 1830: and for later catalogues, between $+ 0.07^s$ for Cambridge, 1860; and $+ .02^s$ for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British Nautical Almanac, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralize the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0.10^s, — a quantity which can be readily detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors, which follow nearly the same law as those which characterize English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neu-

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tralize the errors inherent in distinctively English and French observations.

d. In the case of several catalogues, residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instruments with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law, and have nearly the same magnitude, as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British Nautical Almanac and the Connaissance des Temps are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860, the ephemerides of the Nautical Almanac were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon Nautical Almanac clock stars. At English observatories, it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the American Ephemeris follow nearly the same law as the errors of the Nautical Almanac, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfers' *Tab. Reg.*, upon which the Berliner Jahrbuch is based, has no well-defined systematic errors; and the correction for equinox is nearly the same in amount as in the Americam Ephemeris, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the American Ephemeris.

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average

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error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox.

		Average error for each star.
Argelander	1830	1.1
Pulkowa	1845	1.1
Greenwich	1845	2.0
Greenwich	1860	2.0
D'Agelet (Gould)	1783	2.2
Cape of Good Hope (Henderson)	1833	2.2
Greenwich	1850	2.2
Greenwich	1871	2.2
Paris	1867	2.4
Washington	1846–52	2.5
Struve	1830	2.5
Cape of Good Hope	1856	2.8
Radcliffe	1860	3.1
Greenwich	1840	3.1
Bessel	1825	3.2
Pond	1830	3.7
Gillis	1840	3.8
Madras (Taylor)	1830	3.9
Cape of Good Hope (Fallows)	1830	3.9
Radcliffe	1845	4.5
Armagh	1840	5.0
Piazzi	1800	5.3
Bessel's Bradley	1755	7.9
Lalande	1800	13.2
Lacaille	1750	24.9

It is obvious from these relations, that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevents any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which it is now well nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different instruments, and by different observers. He says, "With all these precautions, we do not find, by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of

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refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. Nachr.*, nos. 1532–1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in Publication xiv of the Gesellschaft, is no less than 1″.2. The present difference outstanding between the Pulkowa and Greenwich systems at 10° south declination is 1″.7.

Within the past five years, the labors of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in case of a large number of stars; but the different systems still differ systematically, *inter se*, by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the Gesellschaft at its session in Leipsic in 1865, Argelander, who was then president of the society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. Nachr.*, vol. i, 257). It was in pursuance of this plan that the zones between $-15°$ and $+15°$ in declination were observed. These zones were to form the groundwork of the Berlin charts; and Argelander, in the execution of the Bonn Durchmusterung simply carried out the second part of Bessel's recommendation.

These two great works — the second being a continuation of the first, under a better and more feasible plan — are the only ones in existence which give us any knowledge of the general structure of the stellar system, with the exception of the observations of Cooper at Makree observatory, and the charts of Chacornac.

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The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the groundwork of the Berlin charts. The coördinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The Durchmusterung undertaken by Argelander at Bonn was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the coördinates of 324,198 stars situated between $-2°$ and $+90°$ declination. It includes stars to the 9.5 magnitude, the coördinates being given to tenths of seconds of time, and the declinations to tenths of minutes of arc.

The first definite proposal of the work undertaken by the Gesellschaft, however, appears to have been made by Bruhns. In the course of a report upon the operations of the Leipsic observatory, he stated, that, in his view, the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles but he proposed at the same time that the position of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution:—

"The Astronomische Gesellschaft regards it as needful that all the stars to the ninth magnitude, occurring in the Durchmusterung, should be observed with meridian circles, and commissions the council to arrange for the execution of the work."

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This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realization of a plan of organization which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published and presented to the society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the society, he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve; as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged; but he felt obliged to ask the society not to require from him the immediate execution of the plan, but to intrust the serious consideration of it, and the preparation for it, to his zealous friends in the council.

Upon the motion of Struve, the society, by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867, Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the proceedings of the Gesellschaft; but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session; and their report, which is published in the place of the paper presented by Argelander, is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give anything more than a general abstract of it. Since it differs in a few minor points from the first report

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of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows: —

a. The limits in declination of the proposed series of observations are $-2°$ and $+80°$. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit $+80°$ was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the Durchmusterung to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Koenigsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact coördinates of these are to be determined at Pulkowa, thus securing the unity necessary in order to connect in one system the observations of different zones.

d. Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work, it will be desirable to use only three or four transit threads, and only one or two microscopes.

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In order to facilitate the reductions to apparent place, the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours, and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument, one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient, if the change in position during the observations can be interpolated to 0.2″.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and one-half or at the most two hours are not advisable, first, because the zero points will be too far apart, and secondly, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report, all the astronomers present who were willing to take part in this work were requested to communicate with the council, stating the region of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipsic and Mannheim, signified their intention to share in the work. Leiden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the Durchmusterung, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations.

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Preparations for the work of observation were immediately commenced; and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year, the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of Hauptsterne consisting of 336 stars to the fourth magnitude, had been previously observed at Pulkowa by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of Zusat-sterne consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkowa observatory by the zone commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted, each star was observed eight times,—four times in each position of the instrument. The observations were differential with respect to the Hauptsterne.

The results were published by Struve in 1876; and the places there given were used in the first reduction of the Harvard College observations for 1874–75, and perhaps in some other cases also.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkowa. The zone commission established by the Gesellschaft, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and most perfect catalogue of fundamental stars yet published. The Pulkowa system for 1865 was adopted as the basis; but, in order to obtain greater freedom from accidental stars, the final catalogue was obtained by combining with the Pulkowa series, the Greenwich observations from 1836 to 1876, the Harvard College observations for 1871–2, the Leipsic observations, in declination only, between 1866 and 1870, and the Leiden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkowa system.

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The following observatories have taken part in the zone observations:—

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
Nikolaiew	— 2° to + 1°	Lund	+35° to +40°
Albany	+ 1 " + 5	Bonn	+40 " +50
Leipsic	+ 4 " +10	Harvard College .	+50 " +55
Leipsic	+10 " +15	Helsingfors . . .	+55 " +60
Berlin	+15 " +25	Christiana . . .	+65 " +70
Cambridge (Eng.) .	+25 " +30	Dorpat	+70 " +75
Leiden	+30 " +35	Kasan	+75 " +80

The zone between —2° and +1° was originally undertaken at Palermo, that between +1° and +4° at Neuchâtel, that between +4° and +10° at Mannheim, and that between +35° and +40° at Chicago.

In the latter case, the great fire at Chicago crippled the resources of the observatory to such an extent, that Safford was compelled to relinquish the work, which was at that time quite far advanced.

The chief items in connection with this work are found in the accompanying tabular statement.

Attention was called, at an early date, to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the Durchmusterung to the tropic of Capricorn. This was undertaken by Schönfeld.

In the report to the Gesellschaft at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hinderance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated, that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkowa system by a connecting link (Mittel-

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:le Reader.	Observations begun.	Observations completed.	Whole No. of observations of zone stars.	Whole No. of observations of fundamental stars.
:i.....	June 1877	—	4400	800
....	Aug. 1878	Aug. 1882	20480	3000
.....	—	Apr. 1883	35000	400 stars observed.
·g	June 1869	June 1872	22532	2940
ɔin	Nov. 1879	Feb. 1883	24107	3556
C	— 1872	—	10332 stars observed.	74 stars observed.
..... nor..... ındo Bakhuyzen. ..... yn.....	Sept. 1870	May 1876	21000+	2500
ɔm ..... ist.....	Apr. 1879	Oct. 1882	23147	2400
..... ns ..... s .... ɜr	Mar. 1869	—	41000	6300
Ha nell ..... mack..... k.....	Nov. 1870	Jan. 1879	22400	15200
r.....	Aug. 1869	Aug. 1880	33756	4 or more stars for each zone.
yden.....	Feb. 1870	Sept. 1881	11019	1861
— — — —	Mar. 1870	June 1882	3400 stars observed.	—
—	—	—	—	—

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glied), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuyzen, at Leiden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the coöperation of Melbourne; and Peters suggested the advantage of securing Washington as an additional "mean term" (V. J. S., 1877, p. 265).

The next reference to this work is contained in the Vierteljahrsschrift for 1881, xv, p. 270. A list of three hundred and three southern stars is here given, whose exact places were at that time being determined at Leiden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuyzen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of eighty-three southern fundamental stars by Auwers appears in this number of the Vierteljahrsschrift. The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard College (Safford) for 1864. For stars not observed at Pulkowa, the general catalogue of Yarnall (1858–61), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkowa system for 1865. It is understood that the coördinates of the list of three hundred and three stars are to depend upon this extension of the general system of Publication xiv, to the limits required by the southern Durchmusterung of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude, and involving the difficulties, of the scheme of observations undertaken under the auspices of the Gesellschaft. The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working-list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind have been made to determine the systematic relations existing between the coördinates of each zone with those of its neighbor.

It is probable, however, that the experience of Gill will be

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repeated on a larger scale. In 1878 he solicited the coöperation of astronomers in the determination of the coördinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix, p. 99, of the Monthly Notices of the Royal Astronomical Society. Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0.24ˢ in right ascension and 2″.3 in declination. In four cases the difference in right ascension exceeds 0.30ˢ, and in four cases the difference in declination exceeds 3″.0.

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found:—

Authority.	Δ α	Δ δ	Authority.	Δ α	Δ δ
	ˢ.	″		ˢ.	″
Koenigsberg . .	+.005	−0.71	Leiden . . .	−.053	−0.19
Melbourne . . .	+.026	−0.49	Paris	+.055	+0.01
Pulkowa . . .	+.005	+0.36	Washington . .	−.120	+0.78
Leipsic	+.049	+0 40	Harvard College,	−.072	+0.09
Greenwich . . .	+.009	−0.56	Cordoba . . .	−.032	−0.20
Berlin	+.044	+0.67	Oxford . . .	+.076	+0.21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations, viz.: the difference between right ascensions determined by the eye-and-ear method, and those determined with the aid of the chronograph.

The programme of the Gesellschaft makes no provision for the elimination of errors which depend upon the magnitude of the stars observed; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard College observatory, the direct effect of a reduction of the magni-

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tude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Besides this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leiden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the speaker, two other sources of error have been detected. It has been found, that there is a well-defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavorable atmospheric conditions differ systematically from those made under favorable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about .08^s too great, and that the observed declinations are about 0″.8 too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed, or may yet reveal. The labor involved in this supplemental survey would not be very great, since the object sought would be accomplished by a special programme of observation, involving perhaps one-tenth of the number of observations made in the present survey. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of sidereal motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all, it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system, we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis

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of universal gravitation. Do the same laws pervade the interstellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion? or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system? or did Newton indeed pierce the depths of the universe in the discovery of the law which gave him immortality? Are we to take the alternative stated by Ball, — either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal interpretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and α Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for anything we can tell, may be practically regarded as infinite?" Can we assert with Newcomb, that in all probability the stars do not form a stable system in the sense in which we say that the solar system is stable, that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear or curved, to say nothing of the laws which govern that motion? How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Mädler, that Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the Gesellschaft, and in the survey of the southern heavens at Cordoba by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the si-

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dereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to affect an elimination of the accidental errors of observation, without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First: The proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted, the derived proper motions are functions of this element.

Second: The general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motions at any point, the effect of these errors upon the values of the coördinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the stellar systems independently determined by Auwers, Safford, Boss and Newcomb, we find the following deviations *inter se* at the end of a century.

	Maximum mean deviation in a century.		Maximum systematic deviation in a century.	
	$\Delta\alpha$	$\Delta\delta$		
Auwers *minus* Safford . . .	*—0.22s.	+0.2″	0.23s.	1.1″
Auwers *minus* Boss	–	+0.8	–	2.1
Auwers *minus* Newcomb . .	—0.09	+0.8	0.06	2.2

*With Bessel's constants of precession.

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It is the common impression, that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situate on the right line which joins the two stars of the third magnitude π and ω Herculis at a quarter of the apparent distance between these stars measured from π Herculis. The velocity of this motion is such that the sun, with the whole cortége of bodies depending on him, advances annually in the direction indicated, through a space equal to 1.623 radii of the terrestrial orbit or one hundred and fifty-four millions of miles."

It must be admitted that there is a general agreement in the assignment by different investigators of the coördinates of the solar apex. This will be seen from the following tabular values:

Authorities.	Right ascension.	Declination.
Herschel, 1783	257° 00′	+25° 00′
Prevost	230 00	+25 00
Klugel, 1789	260 00	+27 00
Herschel, 1805	245 52	+49 38
Argelander, 1837	259 52	+32 29
Lundahl	252 24	+14 26
Struve	261 23	+37 36
Galloway	257 04	+34 18
Mädler	261 38	+39 54
Airy	256 54	+39 29
	261 29	+26 44
Dunkin	261 14	+32 55
	263 44	+25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz., upon Bradley.

(*b*) The deviations *inter se* probably result, in a large measure, from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Biot in 1812, Bessel in 1818, and Airy in 1859, reached the conclusion that the *certainty* of the movement of the solar

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system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a member of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data, nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of coördinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motion were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condition are formed from which a correction is found to the assumed position of the apex, by the method of least squares.

It must always be kept in mind, that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume 0″.2 as the limit of precision in the absolute determination of the coördinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present date of instrumental astronomy. It is safe to say, that there is not a single star in the heavens whose coördinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which the error will at some future time be found to fall within this limit. But who is prepared to select a particular star, and say that the absolute position of this star in space cannot be more than 0″.2 in error?

(*e*) At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford in his investigation made use of a device which he had previously employed in the computation of the differential coëfficients of planetary elements, thereby reducing to a certain ex-

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tent the magnitude of the residuals between observation and computation to quantities of the second order. The axis of Z was located in the direction of Argelander's determination of the apex of the solar motion or in right ascension 259° 50′.8 and declination +32° 29′.1. The axis of X was placed in the equator in right ascension 349° 50′.8 and that of Y in right ascension 79° 50′.8 and in declination 57° 30′.9. When the reductions to these three axes were made, it was found that nearly all of the motion was along the axis of Z as was to be expected from the assumption made. The tendency to move in this direction was therefore primarily assumed to represent the solar motion. Since it was found that the *average factor*, by which the proper motion must be multiplied in order to represent this tendency, changes only in a slight degree with the magnitude of the proper motion itself, the proper motion was assumed, as a first approximation, to be inversely proportional to this element.

Later investigations have been made by De Ball (Ueber die eigene Bewegung des Sonnensterns, Bonn, 1877) but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

The most recent investigation is by Bolte. His paper may be considered as a numerical application of the formulæ by which Schönfeld has attempted to express the relation between the motion of the solar system and a systematic rotation of the fixed stars which is assumed to take place in a direction perpendicular to the plane of the Milky Way. The values of the precession constants derived are certainly interesting, but they can hardly be considered decisive, especially in view of the fact that the values derived from the observed declinations alone do not agree especially well with the values derived from the right ascensions.

It is clear from this brief review, that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the Gesellschaft. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and to prepare the way for a more perfect scheme of observation in the next century.

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PAPERS READ.

THE TOTAL SOLAR ECLIPSE OF MAY 6, 1883. By EDWARD S. HOLDEN of Madison, Wisconsin.

[ABSTRACT.]

THIS eclipse had the longest totality of any which has been observed.

An expedition was sent by the National Academy of Sciences and the United States Coast Survey jointly, under direction of a committee from the former. Expenses were met by an appropriation of $5,000 by Congress and by the National Academy of Sciences from a fund left by Professor Watson. The navy department also placed the United States steamer Hartford at the disposal of the academy, to transport the expedition from Peru to Caroline Island, where the eclipse was to be observed, and thence to Honolulu.

The efforts of Mr. Rockwell to provide money by private subscription for this undertaking, though directly unsuccessful, prepared the way by drawing public attention.

Professor Young was the chairman of the committee of the National Academy of Sciences: it was at one time hoped that he would take charge of the observing party, but this proved impracticable. The reports of different members of the party are to be submitted to the National Academy of Sciences in November. Mr. Holden has, however, permission of the academy to present an account of the observation before the American Association. It is understood that the present is not by any means a final report. This especially applies to the observations of Dr. Hastings, from which that gentleman concludes that the solar corona is chiefly a phenomenon due to the diffraction of the solar light at the moon's limb. The computations to demonstrate this are not yet at hand, but are to be completed in a few weeks.

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The American party consisted of Edward S. Holden, director of Washburn Observatory, Madison, Wis.; Charles S. Hastings, professor of physics in the Johns Hopkins University, Baltimore, Md.; Charles H. Rockwell, Tarrytown, N. Y.; E. D. Preston, aid U. S. Coast and Geodetic Survey, Washington, D. C.; Winslow Upton, U. S. Signal-office, Washington, D. C.; and Ensign S. J. Brown, U. S. N., U. S. Naval Observatory, Washington, D. C.

The original six members of the party were joined, on April 20, by four volunteer observers, all officers of the U. S. ship Hartford: these were Lieut. E. F. Qualtrough, U. S. N.; Passed Assistant-surgeon W. S. Dixon, U. S. N.; Midshipman W. S. Fletcher, U. S. N.; and Midshipman J. G. Doyle, U. S. N.

On March 11 the party was strengthened by the joining (at Colon) of the two English gentlemen who were sent out by the Royal Society of London to make photographic observations of the eclipse, under instructions from J. Norman Lockyer, Esq., F. R. S., and Capt. W. de W. Abney, R. E., of the Science and Art Department of the South Kensington Museum. These were H. A. Lawrance, London, Eng., and C. Ray Woods, London, Eng.

During the stay of the party on Caroline Island (April 21 to May 9) ten petty officers and men of the Hartford remained, and rendered very intelligent assistance.

In all, the party on the island consisted of twenty-two persons.

After giving details of the proceedings of the expedition, its arrival, and the preparations for the eclipse, Mr. Holden states, as to the event itself, that the following atmospheric conditions prevailed: the sky proved clear at first contact, cloudy at intervals till near totality, clear during totality except a slight haze during its first minute, cloudy a few minutes after third contact, and finally clear at fourth contact.

The meteorological observations were made by Mr. Upton, and are especially noteworthy. In two weeks, April 25 to May 9, twenty showers were recorded; but the rainfall in each was very small, the total in the two weeks being about eight inches. Half of this fell during the only considerable disturbance of the weather, which took place May 4, when it rained from midnight to 9.50 A. M.

The barometer was notably uniform. Its diurnal movements were plainly marked; the maxima being at 9 A. M. and P. M., the minima at 3 A. M. and P. M. The indications of the thermometer

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were very constant. The daily range was 9.3°, the highest reading was 89.3°, the lowest 72.4°, the daily maximum at *noon*, the minimum at 6 A. M. The relative humidity ranged from 70 per cent at midday to 84 in early morning, and at no time fell below 61. The island lies in the region of the south-east trades, but the wind (which was very steady) blew constantly between north and east. The average velocity of the wind was 6.05 miles; the largest during twenty-four hours was 212 miles, the least 59 miles; the highest velocity, registered in a squall, was 16 miles per hour.

The botanical and zoölogical observations are not yet ready for publication. During the voyage a series of observations was made by Mr. Upton on southern variable stars. Dr. Hastings and Mr. Holden, while on the island, discovered twenty-three new double stars, a list of which has appeared in SCIENCE.

In preparing for the eclipse, Mr. Holden assigned to each observer a single duty, not requiring him to move from one instrument to another. The entire field of photography was left to the English party accompanying our own, and to the French party under M. Janssen, who were very successful in photographing the corona.

The combination of polariscope and telescope was used, but not with successful results, the apparatus proving unsuitable. Dr. W. S. Dixon, who attended to a telescopic examination of the details of the inner corona, will report on the same separately, giving a drawing of the corona. With the spectroscope, the chief point of the observation of Dr. Hastings was as to the relative lengths of the line 1474 east and west of the sun. At second contact, this line was 12′ in length east and 3′ west. The length of 1474 east diminished, while 1474 west increased. At mid-totality these were equal. Before the third contact, the appearances were reversed: 1474 west was longer and brighter than 1474 east.

At the beginning of totality, the lines C, D_3, F, and (near G) were seen brilliant but very short. At mid-eclipse the spectrum was deliberately examined. On a continuous spectrum, two lines only were seen: 1474 *bright*, and the D line *dark*. C, E, b, F, were certainly wanting. Near the end of totality, C, D_3, and F appeared again, very short. Mr. Upton reports that five seconds after second contact, four curved lines were seen,— C, D_3, 1474, F. A light cloud passed over the sun; and on its disappearance the

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spectrum showed a small line, of about one-third the height of the others, between 1474 and *F*. One hundred seconds after second contact, three coronal rings took the place of the lines; they were red, yellowish-green, and green, and are supposed to be *C*, D_3, and 1474. Two hundred seconds after second contact, the red ring was decidedly the brightest, and it continued to increase in brightness during sixty seconds. Two hundred and ninety seconds after second contact, the four curved lines, *C*, D_3, 1474, *F*, appeared. The reversal of the bright lines at third contact was observed. The change was instantaneous, or nearly so. The reversal of the Fraunhofer lines was not seen. The only bright line seen for the first 190 seconds was 1474. A dark line was seen, which was probably *D*.

Mr. Rockwell, using a Rutherford grating and a narrow slit tangential to the limb, reported that 1474 *K* was not seen until a minute and a half had passed. It was followed 4′ or 5′ west of the limb, twice; and it was seen only on the western side of the moon. Two green lines were also seen, each brighter and broader than 1474, but much shorter.

Due credit is given by Mr. Holden to each of the observers of the party. His own observations were confined to a search for the planet Vulcan, reported to exist by Professors Watson and Swift. Mr. Holden's search continued during the whole of totality (five minutes and twenty-five seconds), with a six-inch telescope with a power of 44 and field of 57′ in declination. He saw every star on the map which he had previously published in SCIENCE (Feb. 23, 1883), down to the sixth magnitude, inclusive, except the three sixth magnitude stars nearest to the sun; and he saw only these stars. One of the stars of the map was of the same magnitude as Watson's "Vulcan." This was a conspicuous object. No star half so bright as this could possibly have escaped observation. Mr. Holden is therefore confident that Vulcan does not exist within the limits swept over. Mr. Holden also determined the direction of the motion of the diffraction bands before and after totality. This was an observation which he could not make successfully in Colorado in 1878, and which he believes has not been before made

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Internal Contacts in Transits of the Inferior Planets. By J. R. Eastman of Washington, D.C.

[ABSTRACT.]

From the general discussion which followed the announcement of the results from the modern reëxamination of the observations of 1761 and 1769 and of the methods employed in obtaining them, it became unpleasantly certain that the numerical value of the solar parallax depended chiefly upon the computer's interpretation of the observer's record.

From the earliest observations most of the observers had been more or less baffled by a phenomenon generally known as the "black drop" or "dark ligament" which, at or about the expected times of internal contact, had appeared between the limbs of the Sun and Venus, and rendered the recorded times of the contacts uncertain to the extent of fifteen or twenty seconds.

As the "black drop" was generally seen by those who observed the transits of the eighteenth century, the interpretation of the recorded observations was the principal problem which challenged the skill of those astronomers who attempted a new discussion of the observations of 1769.

Powalky seems to have adopted no rule for explaining the meaning of the records, rejecting such as he deemed discordant, and in 1864 he deduced a new value of $\pi = 8''.832$.

Stone, after a careful study of all the records, adopted a rigid rule for interpreting the records in all cases where the "black drop" was seen and from his discussion obtained the value $8''.91$.

He concluded that the "black drop" or some modification of it was a necessary phenomenon whenever a black disk like Venus or Mercury was seen against the bright surface of the Sun.

In 1868, Mr. Stone said:[1] "The apparent diameter of the Sun is increased, and the apparent diameter of Venus, while on the Sun's disk, is decreased from the same causes which produce the spurious disk of a star in the focus of a lens. When the true limb of Venus falls in a line with the true limb of the Sun, the light is cut off; the spurious enlargement of the Sun's disk and encroachment on that of Venus is destroyed at the point of contact, and, as a consequence, a dark ligament appears to connect

[1] Monthly Notices R. A. S., vol. XXIX, p. 47.

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the apparent limbs of the Sun and Venus. This is the real internal contact. The breadth of the connecting ligament rapidly increases, and, after some eighteen seconds, the exact interval depending upon the circumstances of the transit and the success in first catching phase (1), the limbs of Venus and the Sun will appear in contact. This is the second phase for accurate observation, the *apparent* internal contact."

This quotation from Mr. STONE expresses very fairly the opinion of those who believe that the "black drop" is, from the nature of the circumstances, a natural and *necessary* phenomenon.

In 1876, M. ANDRÉ,[2] the astronomer who had charge of the French expedition to Nouméa, in 1874, to observe the transit of Venus, announced as the result of analytical investigation and of a long series of experiments made by himself, that:

"The bridge, black ligament, or black drop as it is variously called, is a necessary phenomenon under certain circumstances, and not merely accidental. With an unchanging source of light of sufficient brilliancy the angular dimensions of the ligament are inversely proportional to the diameter of the object-glass, and with an aperture of five inches the drop is scarcely perceptible. Diaphragms should never be used when observing Venus in transit, as the apparent dimensions of the drop are augmented by any increase in focal length.

"It is always possible to get rid of the ligament and reduce the phenomena to "geometrical contacts, either

(*a*) by reducing sufficiently the intensity of the source of light, or augmenting the absorbing power of the dark glass employed: or, again

(*b*) by covering the object-glass with a network diaphragm composed of rings alternately full and empty, and all very thin, but bearing a certain proportion to the focal length of the lens."

These results and opinions of M. ANDRÉ were not generally known at the time of the transit of Mercury in 1878, though ANDRÉ travelled from Paris to Ogden in Utah to test his results, from analysis and experiment, by actual observation. His theories were confirmed by his observations in 1878, and in 1881 he published his results more fully together with the details of his experiments.

In 1878 the necessity of the "black drop" phenomena was gener-

[2] Monthly Notices R. A. S., vol. XXXVII, p. 58.

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ally recognized, and it was seen by many observers of the transit of Mercury. Some who saw it evidently regarded their success in finding it, an evidence of the good quality of their observations. In a few cases observers apologized for *failing* to see the black drop.

One fact, however, was worthy of notice, that *every* observer, so far as I could ascertain, who was accustomed to observe the limbs of the Sun for position; or, in other words, to get by means of shade glasses the best definition of the Sun's limbs with an illumination somewhat *less* than the eye could easily bear,—*did not* see any trace of the black drop.

Although I had then seen no account of ANDRÉ's experiments and had given but little attention to his deductions as announced in the Royal Astronomical Society by Father Perry, I was convinced after my observation of the transit of *Mercury* in '78, that the theory of a *necessary* "black drop" was fallacious. As I had been able to make only one experiment, the data to sustain my belief, from my own observations, were very meagre and I waited four years for another opportunity. Many other observers in 1878 failed to see the obstructing phenomenon, but out of the various descriptions of the contact observations I did not find the *exact* data which I desired as proof.

The experience gained in the observations of May 6, 1878, and from the subsequent discussions of the results, were probably not without their effect in the observations of the transit of Venus in Dec., 1882, for I have yet to learn of any observer in the eight American parties organized by the Transit-of-Venus Commission who saw any trace of the "black drop;" while in 1874 many of them report such phenomena.

Last December, at the Transit-of-Venus station at Cedar Keys, Florida, the observation of *first* contact was prevented by a cloud which covered only a small portion of the Sun's disk. This cloud soon disappeared and the illumination was reduced, with the sliding shade-glass, until the brightness was considerably less than could be easily borne by the eye, while the definition of the Sun's limb was perfect. The Sun was surrounded by haze, and thin cirri which increased the steadiness and definition of the limb and permitted the use of a less dense shade-glass.

The definition and steadiness of Venus were all that could be desired, and there was no modification, at the limbs, of the dense black color of the disk.

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The objective of the telescope was *five* inches in diameter, and the whole aperture was used. The eye-piece magnified 216 diameters, and was used in connection with a Herschel solar prism. The shade-glass was a wedge of neutral tint mounted against a similar wedge of plain glass in a thin brass frame, the whole sliding freely in a slotted cap for the eye-piece.

My attention was of course directed, *first*, to fixing the exact time of contact; *second*, to looking for the black drop and its modifications, or for any change in the tint of the Sun's limb at the point of contact.

The entire disk of Venus was visible for several minutes before *second* contact, and the portion beyond the Sun's disk was bordered by a narrow line of light much less *bright* than the limb of the Sun, and of a lighter tint.

The progress of the transit was strikingly barren of notable events. About one minute before the contact, the apparent motion of the cusps of the Sun as they closed around the planet was noticeably increasing, though the movement was the perfection of steadiness.

I had not long to wait, for the cusps rapidly swept around the body of the planet in a perfectly steady line of sunlight of the same tint as the adjacent parts of the Sun. This line was as narrow as I could see with a power of 216, and was free from any tremors or pulsations.

There was not the slightest agitation of the limbs of either body near the point of contact, and no *trace* of any *black drop*, *ligament* or *band*.

There was no change of color or tint on the limb of Venus near the point of contact, and no indication of any *clinging* of the limbs.

The contact was as easily and I think as accurately observed as the transit of a star within 8° of the pole under the best conditions. The uncertainty of noting the time of the visible contact could not be greater than *three-tenths* of a second.

The phenomena at the *third* contact were similar to those at the *second* in the reversed order.

From my own experience in 1882 and in 1878, as well as from the work of other observers, I am thoroughly convinced that with a properly arranged telescope and shade glasses, no good observer need have any trouble from any phase of the "black drop." It must not be forgotten that the observer of contacts has but a

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single purpose in view. He has no time to devote to the study of any branch of solar physics; he is simply to obtain a good definition of the *Sun's* limb as a reference point in the passage of the limb of *Venus*. Searching for some *new thing* detracts from the value of his proper work.

If it be urged that the light should be as strong as the eye can bear, thereby producing all the troublesome phenomena that diffraction or irradiation will give at the limit of vision, why not simplify the process by discarding shade glasses and all aids to good vision, and see,—*nothing* that is trustworthy.

Observations de M. J. Janssen, Directeur de l'observatoire de Meudon, chef de la mission francaise a l'Ile Caroline pour l'observation de l'éclipse totale du 6 Mai, 1883. Communicated by Prof. J. R. Eastman of Washington, D. C.

OBSERVATIONS SPECTROSCOPIQUES.

Le but principal des observations de l'auteur était l'étude des raies obscures de la couronne.

D'après l'avis de l'auteur la visibilité de ces raies dépend beaucoup plus du pouvoir lumineux de l'instrument que de la perfection des images.

D'après ces vues l'auteur avait fait construire un téléscope (reflecting) dont le miroir a 50 centimêtres de diamêtre et seulement 1^m 60 de foyer. Dans cet instrument les images de la lune sont d'une intensité extraordinaire et qui blesse la vue. Avec ce téléscope on a pu obtenir de bonne images de la lumière cendrée en une minute d'exposition. C'est cet instrument qui a été employé à Caroline. Le spectroscope adapté au téléscope est formé d'un prisme à vision directe contenant les prismes de matière très pure. Le collimateur a un foyer et une ouverture calculés pour que tous les rayons envoyés par le miroir soient utilisés par lui.

Un chercheur très précis est disposé sur l'instrument de manière que l'un des yeux étant au chercheur l'autre œil puisse avoir l'analyse du phénomène.

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Avant la totalité complète le miroir du téléscope était couvert en partie pour éviter que les rayons solaires ne brissent le prisme réflecteur qui renvoie la lumière dans le spectroscope.

Dès que la totalité a été presque complète le miroir a été complétement découvert, et on a commencé l'exploration de la couronne.

On a reconnu d'abord les raies brillantes que la couronne présente ordinairement mais, ce qui a été nouveau et plus complet qu'on ne s'y attendait même, c'est que le fond du spectre coronal a présenté le spectre *Fraunhoférien complet;* c'est-à-dire, les raies obscures du spectre solaire moins celles de ces raies qui deviennent brillantes dans la couronne, comme C. F. etc., et celles qui par leur finesse ne sont visibles que dans les spectroscopes extrêment dispersifs. Et ce qu'il faut bien remarquer c'est que dans les circonstances d'éclairement ou on était placé, toutes les raies obscures du spectre solaire qu'il était théoriquement possible de voir ont été vues.

J'ai même été si frappé de l'évidence du phénomène que j'ai demandé à M. Trouvelot, qui observait à côté de moi de venir le voir. Mais ces occupations l'en ont empêché. Ce phénomène remarquable a été observé dans les parties les plus brillantes de la couronne, mais non d'une manière égale à même distance du limbe lunaire. Ceci indique qu'il y a dans la couronne des parties qui réfléchissent beaucoup plus abondament la lumière émanée du globe solaire, circonstance en accord avec l'existence autour de ce globe de matières cosmiques circulant autour de lui.

Nous ne développerons pas ici toutes les conséquences du fait constaté à Caroline, nous voulons surtout constater l'existence dans le spectre coronal du spectre de Fraunhofer complet, nous réservant de revenir sur les conséquences de ce phénomène, relativement à la constitution de la couronne et des espaces circumsolaires. J'ai trouvé le spectre de la couronne continu dans certaines régions basses de la couronne.

J'ai étudié aussi les anneaux de Respighi. Ces anneaux ne se sont pas montrés réguliers autour du limbe lunaire; ils ont présenté deux particularités de structure qui montrent la complexité du phénomène coronal et qui seront discutées plus tard.

La lumière de la couronne était vivement et radialement polarisée.

Toutes ces études: examen des formes de la couronne, raies

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obscures anneaux de Respighi, polarisation étaient associées en vue de la question des matières cosmiques circumsolaires, nous pensons que les résultats obtenus à Caroline et spécialement la constatation du spectre fraunhoférien dans les régions circumsolaires avancent cette question.

PHOTOGRAPHIE.

J'avais emporté de France deux grands appareils sur lesquels étaient montés huit chambres photographiques, dans le but d'étudier la question des planètes intra-mercurielles et celles des formes de l'étendue de la couronne.

Vu des appareils photographique avait un objectif de 8 pouces d'ouverture qui embrassait une région de 15° tout autour du soleil. Cette appareil donne, par une belle nuit, des étoiles de 6 et 7e grandeur en quelques minutes. Il était spécialement destiné à Caroline, à la recherche des planètes hypothétiques intra-mercurielles. Un premier examen des plaques permet de dire que rien d'important à ce point de vue ne s'est montré autour du soleil pendant l'éclipse totale.

Pour moi la non existence de planètes importantes intra-mercurielles me semble démontré, sinon d'une manière absolue au moins avec une probabilité extrêment considérable. Les autres appareils photographiques avaient des pouvoirs lumineux très différents et étaient destinés à résoudre plusieurs questions nouvelles sur la constitution de la couronne.

Ces appareils ont montré que la couronne est un phénomène délimité et qu'au delà d'un certain temps de pose *son image photographique n'augmente plus en étendue.*

Vue des images de la couronne a été obtenue avec un objectif de 4 pouces à court foyer et qui a agi pendant cinq minutes complète sur une plaque à la gélatine très sensible. La couronne y est nettement limitée et d'étendue sensiblement égale à celle obtenue avec des actions lumineuses beaucoup plus faibles.

Les formes de la couronne se sont montrées fixes pendant toute la durée de la totalité. En résumé mes observations à Caroline tendent à établir: qu'il n'existe point de planètes ultra-mercurielles importantes, et que les perturbations reconnues par Leverrier dans les mouvement de Mercure, doivent plutôt être recherchés dans la présence de matières cosmiques circulant autour du soleil.

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ORBIT OF THE GREAT COMET OF 1882. By EDGAR FRISBY of Washington, D. C.

[ABSTRACT.]

THIS is a partial record of observations at Washington. Mr. Winlock of the U. S. Naval Observatory is preparing a description of all the physical phenomena of the comet which were there observed. The first observation of the comet made at Washington was with the 9.6 inch Equatorial at about two o'clock on the afternoon of September 19, comparisons being made with the sun for the position of the comet. Good meridian observations were obtained on September 20 and 21, and from the equatorial observation on the 19th and the two meridian observations of the 20th and 21st, calculations were made which served to fix the place of the comet with fair approximate accuracy for three months, which was a somewhat remarkable success. After the fifth of October a difficulty occurred in obtaining accurate observations, on account of several different points of light being presented in an ill-defined nucleus, and it was uncertain whether the observations always referred to the same luminous point, the nucleus sometimes extending over as much as four seconds of time in right ascension and as much as three or four tenths of a minute in declination.

The following elements were computed from observations made at the U. S. Naval Observatory on September 19, October 8 and November 24, astronomical time, the first and last being made on the meridian and the middle one on the equatorial.

Perihelion Time				Sept. 17.^{d}2282		Wash. M. T.
$\Omega =$	346°	1′	7.91″	Φ		89° 13′ 42.70″
$\pi - \Omega =$	69	36	12.79	log. a	=	1.9331366
$i =$	141	59	52.16	log. q	=	7.8904739
				period	=	793.689 years.

The elements are referred to the mean equinox of 1883.0.

The first accurate observation of this comet of which we have any record was made by Mr. Finlay at the Cape of Good Hope on September 8. Among the last were observations made at the Kempshot observatory, Jamaica, April 26, 27, May 5 and 6 by Mr. Maxwell Hall. The elements compared with the Cape of Good Hope observation give a correction of only half a second of

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time in right ascension and about one second in declination; compared with the Jamaica observations the corrections vary from 0 to 3ˢ in right ascension and from 0.′1 to 1.′8 in declination. Comparisons were also made with other observations which gave a correction sometimes as much as 3ˢ or 4ˢ in right ascension and about 0.′3 in declination; these variations probably being partly accounted for from observing different parts of the nucleus.

The elements were also compared with the computations of other astronomers. The most prominent variation was in respect to the period which others gave as 659, 654, 823, 852 and 997 years. The last period was that computed by Dr. Morrison of the Nautical Almanac Office at Washington.

The difference between 793 and 997 years seems considerable, but the elements agree very closely, the inclination being exactly the same as that here given; the position of the node, and the distance from the perihelion to the node differing only by about 0.′3. The period of 853 years was computed by Dr. Kreutz, that of 823 by Fabritius. A pasteboard model constructed by Mr. Winlock was exhibited, showing the relative positions in space of the earth, sun and comet.

Observations upon Uranus made in 1883, at Princeton, N. J.

By C. A. Young of Princeton, N. J.

[ABSTRACT.]

During May and June, 1883, a series of micrometric measurements upon the planet's disk was made, for the purpose of determining its ellipticity; the time being especially favorable because of the planet's nearness to the node of the satellite orbits. The instrument used was the 23 inch equatorial of the Halsted Observatory, with the filar micrometer: powers of 480 and 720.

For the polar diameter, reduced to the mean distance 19.1826, we obtained from 140 readings on eight different evenings, 3″.974.

For the equatorial diameter, ... 4″.280, from the same number of readings on the same evenings, May 18 to June 19 inclusive.

The ellipticity of the disk deduced is about $\frac{1}{12}$.

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Whenever the seeing was good, faint markings were always visible on the disk of the planet.

They appeared to be dark *belts*, resembling somewhat those of Jupiter. Usually there were two of them, one on each side of the equator, their general trend being commonly in the direction of the major axis of the satellite orbits, though on one or two occasions they seemed to be inclined *sinistrorsum* to this line by some 10° or 20°. Nothing was made out which could give a satisfactory determination of the planet's axial rotation. Once or twice a difference was noted between the brightness of the two polar regions, the western being brighter than the eastern; and on one evening one of the observers (C. A. Y.) suspected a sort of spur running out from the polar edge of the western belt: but the object was very faint, and the appearance never recurred.

The observations were made by Professor Young and Mr. McNeill.

PHYSICAL PHENOMENA ON THE PLANET JUPITER. By G. W. HOUGH of Chicago, Ill.

The rapid motion of revolution of the planet, by changing the positions of the markings on the surface to our line of sight, makes great apparent differences in their shapes and sizes. This has perhaps been the occasion of reports of sudden and great changes upon the surface. The changes are not sudden but are gradual; and many of the features are permanent. Minor changes are constantly in progress in the equatorial belts. The author recently observed the belt drifting down toward the red spot; but although it partly surrounded it, they did not coalesce, and the spot forced a scallop into the belt,—a very curious phenomenon. The author saw a satellite pass over the red spot, though the satellites are not visible when on the white part of the disk. He had also had a chance to compare shadows of satellites on the disk and on the spot, and both are dark. The red spot has seemingly retrograded during the past four years; that is to say, the rotation of Jupiter has seemingly increased from 9 h. 55 m. 33 s., to 9 h. 55 m. 38 s. The future observer should attend more carefully to what he sees, and theorize afterward.

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On a Method of observing the Eclipses of Jupiter's Satellites, employed at the Lawrence Observatory of Amherst College, bearing on the Determination of certain corrections proper to be applied to the early observations of these Eclipses. By David P. Todd of Amherst, Mass.

[ABSTRACT.]

Among the more important corrections which have to be applied to the early observations of the eclipses of Jupiter's satellites to render them homogeneous and comparable, is the correction for aperture of the telescopes used in making the observations. With reference to throwing some light on this question, a method has been devised whereby any given eclipse may be repeatedly observed at either phase by a single observer with varying apertures of his telescope. This method was brought into use at the Lawrence Observatory early in the present year, and the results so far obtained indicate the desirability of continuing the work in the future.

Observations on the transit of Venus made at Columbia College Observatory. By J. K. Rees of New York, N. Y.

Investigation of the Light Variations of Sawyer's Variable. By S. C. Chandler, Jr., of Cambridge, Mass.

[ABSTRACT.]

This paper contains a discussion of the elements and light curve of this variable of the Algol type, from all the available material. The period is shown to be at present 20 h., 7 m., 42.0 s., and the presence of an irregularity in the period is demonstrated by observations at Cordoba and Copenhagen in 1871 and 1863 respectively. Anomalies in the light curve are also shown very probably to exist.

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Results of Tests with the Almucantar, in Time and Latitude.

By S. C. Chandler, Jr., of Cambridge, Mass.

[ABSTRACT.]

The instrument which has been named the "almucantar" was described and figured in a paper presented to the association at its meeting in 1880. In its general nature it is an equal altitude instrument. A hollow rectangular trough containing mercury revolves horizontally on an upright central pillar. The trough contains a float which is perfectly free to obtain equilibrium, while it is constrained to revolve with the trough. The float carries a telescope which turns on a horizontal axis, and can be clamped at any desired altitude. When this instrument is revolved on its vertical axis, any given point in the field of view describes a horizontal small circle, or almucantar, in the heavens. The transits of stars over a series of horizontal lines will thus afford means of determining the altitude of the instrument, the error of the clock, the latitude or the declinations of the stars, by a proper distribution of the observations in azimuth.

A higher degree of accuracy is attainable by this instrument than by a transit or a zenith telescope of the same size. The author's comparison of results is as follows: the probable error of a single star in determining the clock error is only $\pm 0.05^s$ or $\pm 0.06^s$. With a transit instrument of the same size, the quantity is not less than $\pm 0.08^s$. With the almucantar the probable error in determining the latitude from a single star is $\pm 0.55''$, including the error of the star's place. This is about equal to the probable error of a pair of stars by Talcott's method, with the larger telescopes of the United States coast-survey.

The instrument was a small one, — $1\frac{3}{4}$ inches aperture and 25 inches focus. It was constructed for experiment only, in a provisional way, at a cost of $150. There are obvious defects in design and construction: when these are remedied, the errors can be much reduced.

A series of observations with this instrument are given by the author, for the latitude of a pier about 80 feet north of the Harvard-college observatory. The value obtained by averaging these is $0.7''$ less than given by Professor Peirce in his discussion of the prime vertical transit observations taken by the Messrs. Bond, and

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adopted as the standard value of the latitude of the observatory, allowance of course being made for differences between piers. The author concludes that Professor Peirce's value is too large by fully three-quarters of a second. By way of proof the author gives a series of observations on the five stars used by Professor Peirce. These are compared with those of Auwers and Boss, and the correction of the hitherto accepted value of the latitude now indicated by the almucantar is thereby confirmed.

The clock errors of two nights selected at random, as given by the almucantar, were exhibited by the author. The results both in time and latitude would be considered satisfactory with an ordinary instrument of two or three times the size. The almucantar can be made much larger than the one under trial, certainly of five or six inches aperture, with corresponding increase of precision along with greater optical power. Its mechanical construction is simple, and reduces the sources of error. Thus in the older instruments there are involved: 1. The accurate construction of parts as of pivots, level, graduated circles. 2. Fixity of mounting, to avoid a shifting of the instrumental plane. 3. Rigidity of the instrument itself, to secure constancy of collimation and flexure. In the almucantar only the last condition has to be satisfied, and it is by far the easiest of the three to be attained mechanically.

The author regards the principle of flotation adopted as being as delicate an indication of the direction of gravity as is obtained by the spirit-level.

The almucantar gives promise of a new instrumental resource in the higher practical astronomy. It is competent to deal with the most delicate problems. It will evade some of the minute sources of errors that still cling to meridian instruments. Especially, it furnishes a method for obviating difficulties, hitherto regarded as almost insuperable, connected with flexure and refraction, in observations with the meridian circle.

A STANDARD TIME-POINTER AND A TIME LONGITUDE DIAL. By SAMUEL EMERSON of New York, N. Y.

A SYSTEM OF ALGEBRAIC GEOMETRY. By SAMUEL EMERSON of New York, N. Y.

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AN IMPROVED METHOD OF PRODUCING A DARK-FIELD ILLUMINATION OF LINES RULED UPON GLASS. By W. A. ROGERS of Cambridge, Mass.

A NEW METHOD OF INVESTIGATING THE FLEXURE CORRECTIONS OF A MERIDIAN CIRCLE. By W. A. ROGERS of Cambridge, Mass.

THE ROTATION OF DOMES. By G. W. HOUGH of Chicago, Ill.

[ABSTRACT.]

OBSERVATORY domes are in general very heavy. As they grow old, owing to the settling of walls and other changes, they are apt to become almost unmanageable. The dome at Chicago is very weighty, everything about the observatory being built in a very substantial manner. When Professor Hough first tried to move the dome, he found its two sides working with unequal friction; and this was afterward remedied to some extent, but by no means fully. About two months ago a gas-engine was placed in position to revolve the dome. It was a great satisfaction to see the great dome go round continuously, without hitches. The cost of moving the dome by such means is a mere trifle, aside from the first cost of the engine. The use of water power where that was easily accessible must, however, be preferred in many instances where a sufficient head is supplied by street mains.

Dr. C. A. Young said, in discussing the foregoing, that when he came to Princeton he found a very heavy dome there. One man, using thirty pounds pressure on a two-foot crank, was very tired after giving the dome one turn. A gas-engine has since been put in below, and the power is communicated by a belt. A revolution can be made in four minutes, and the shutter raised in two. In general, the dome is placed and the shutter opened within five minutes. Dr. Young expressed a hope that the Brush storage batteries would furnish electrical illumination and power for the work of observatories, as the electricity might be stored even from a gas-engine operating a dynamo during hours of the day when there was no other use for its power. At present the direct action of a gas-engine on a dynamo, with no intervention between the dynamo and the light, was too irregular to serve the purpose.

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DESCRIPTION OF THE NEW OBSERVATORY OF COLUMBIA COLLEGE, NEW YORK CITY. By J. K. REES of New York, N. Y.

[ABSTRACT.]

DESCRIPTION of the observatory as built on iron girders 110 ft. from the ground. Account of the paper dome and the paper transit slit-shutters opening by springs.

OBSERVATIONS ON THE TRANSIT OF VENUS MADE AT COLUMBIA COLLEGE OBSERVATORY. By J. K. REES of New York, N. Y.

THE LIGHT VARIATIONS OF T. MONOCEROTIS. By E. F. SAWYER of Cambridgeport, Mass.

THE CALCULUS OF DIRECTION AND POSITION.[1] By E. W. HYDE of Cincinnati, Ohio.

[ABSTRACT.]

THE paper consists of a comparison of Hamilton's Quaternions with Grassmann's "Ausdehnungslehre," with the design of showing the superior advantages of the latter as a practical instrument for geometrical and mechanical purposes.

First is given a brief sketch of the basis of Quaternions with a statement of its peculiarity in giving to a directed quantity, or vector, in addition to its inherent qualities of magnitude and direction, the *rotative* property possessed by $\sqrt{-1}$

Then a sketch is given of Grassmann's system of geometrical multiplication of points, lines and planes showing its completeness, clearness and adaptability to the uses to which it is to be put, as well as the advantages which, in the opinion of the writer, it possesses over Hamilton's system.

In conclusion the solutions of several problems by the two methods are given in parallel columns to illustrate the relative brevity and conciseness of Grassmann's method, and the condition of static equilibrium is derived as an illustration of the suitableness of the same for the purposes of mechanics.

[1] Published in full in the American Journal of Mathematics. No. I, vol. VI.

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Tide Meter, for use on Soundings at a Distance from the Shore. By John M. Batchelder of Cambridge, Mass.

[ABSTRACT.]

An instrument for ascertaining the rise and fall of the tide, from a vessel at anchor remote from the land, was constructed by order of Prof. A. D. Bache, in 1858, and has been used for this purpose by the U. S. Coast Survey at various places.

It consists of an india-rubber bag filled with air, its capacity being four or five gallons: an india-rubber tube of about one quarter inch internal diameter and a manometer or pressure gauge. The bag is contained in a weighted basket or case, which rests upon the bottom, and the india-rubber tube leads from this through the hawse hole or other opening to the manometer, which is portable and may be placed in any convenient position on board ship.

The instrument is graduated in feet and fathoms, each forty-four one-hundredths of a pound pressure per inch being equal to one foot in depth.

The longest continued series of observations by the pressure tide gauge were made in 1862 and 1863 upon the "Martin's Industry" lightship off Hilton Head, eleven miles from land, in about eight fathoms of water.

The profiles, made from the records of the Coast Survey are of very symmetrical character, in this respect comparing favorably with records of the staff-gauge kept at the same time at Station Creek, thirteen miles distant from the lightship.

With this instrument on board, vessels waiting to cross a bar at the mouth of a harbor may know the exact time of high water.

Construction of certain Problems relating to the General Ellipsoid, by the Method of Descriptive Geometry. By C. M. Woodward of Washington University, St. Louis, Mo.

[ABSTRACT.]

A lemma was explained, which gave methods for readily constructing lines represented by $\frac{x}{a}$, $\frac{x}{a^2}$, $\frac{x}{a^3}$, etc., when x and a are known lines, and it was then shown that for every point x, y, z, on the surface of

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an ellipsoid, there is a corresponding point $\frac{x}{a}$, $\frac{y}{b}$, $\frac{z}{c}$, on the surface of a unit sphere, either of which being assumed the other can be easily found.

From the equations of the perpendicular to a tangent plane the projections of the perpendicular are readily constructed. Hence follows the solution of the problem: *To pass a plane tangent to a general ellipsoid at a given point.*

By means of the expression for the length of a perpendicular to the polar plane of any point the plane is constructed, both when the point is at a finite, and at an infinite distance. Thus are solved problems relating to *lines of shade for diverging and parallel rays of light.*

Finally, by the use of similar methods is solved the problem: *To pass a plane through a given line and tangent to an ellipsoid.*

DESCRIPTIVE-GEOMETRICAL TREATMENT OF SURFACES OF THE SECOND DEGREE. By J. BURKITT WEBB of Ithaca, N. Y.

[ABSTRACT.]

FOR the purpose of greater conciseness the speaker confined his remarks to the general ellipsoid, remarking that the usual treatment of problems upon this surface — as, for instance, such problems as finding the shade and shadow, or drawing tangent planes — is lacking in generality; the body being taken in such special position, or referred to such special axes, as reduce the general problem to a specially simple one.

The speaker then drew the projections of three conjugate diameters of a general ellipsoid upon the board, stating that this was the best method of defining that body. He then proceeded to define the projections of the enveloping cylinders, and the shadow of the body, which he showed could as easily be done for the general ellipsoid in a perfectly general position, as for special cases. In fact, it appeared that problems on this body gained nothing in simplicity by special methods and devices which detract from the generality of the treatment.

CONIC SECTIONS IN DESCRIPTIVE GEOMETRY. By J. BURKITT WEBB of Ithaca, N. Y.

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SOME RECENTLY DISCOVERED PROPERTIES OF SQUARES. By O. S. WESTCOTT of Chicago, Ill.

Methods of extracting the square roots of numbers may be classed as *easy, tedious* and *intricate*.

The *easy* methods depend upon the properties of the square of an algebraic binomial, needs no remark.

The *tedious* method referred to is a little book written by Sam'l Emerson in 1855. It may be indicated thus:

39' 06' 25 625

3

62.

Of necessity, this operation, like the other, depends upon the properties of the trinomial square just mentioned, the only difference in the method being with reference to the point of attack. Thus, in the above operation, 39' 06' 25(6

3

bearing in mind that $(a + b)^2 = a^2 + 2ab + b^2$, after subtracting 6^2 from 39 leaving a remainder 3, take $2a = 12$, for a permanent trial divisor.[2] Then $b = 2$. Now, instead of finding a permanent divisor by noting the possibility of factoring $2ab + b^2$ as $b\ (2a + b)$ and obtaining the value of $2a + b$, remove at once from the power the value of b^2 and subsequently that of $2ab$. The operation will then stand: 39' 06' 25(62

3

62 the mental operation being $b^2 = 2^2 = 4$. $6 - 4 = 2$. $2ab = 2 \cdot 6 \cdot 2 = 24$.[3] $30 - 24 = 6$. A line through the 3 may indicate its removal, and the remainder may be written in the line above if the operator so desires.

The operation then proceeds by determining with our old divisor 12 that the next figure of the root is 5. Then we have:

39' 06' 25(625

3

62 as the completed operation.

[1] Unless otherwise indicated, the word number throughout this discussion signifies integer or integral number.

[2] In the ordinary operation for the extraction of the square root every observer must have noticed that the left hand figures of the several trial divisors rarely change.

[3] Locality rather than local value of 6 being kept in mind without its necessarily being formulated in thought as 60.

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The thought, however, is something more. It is as follows: $5^2=25$. $25-25=0$. Since $2\cdot5\cdot62$ is obviously $(2\cdot5\cdot2)+(2\cdot5\cdot60)$ (see note 2) the operation then proceeds $2\cdot2\cdot5=20$. $20+2=22$. $22-22=0$. $2\cdot5\cdot6=60$. $60+2=62$. $62+62=0$. No originality is claimed for this operation, but by its elucidation, I hope to have called attention to its great practical utility.

I come now to what I have ventured to call the intuitive method. In more than a score of years of school routine in mathematics, I have found it time-saving and advantageous in other respects, to develop a plan of determining at sight, the square root of any perfect square within moderate limits. Expedition in obtaining results, particularly in the elementary matter of quadratic equations is so desirable, that an independent discussion of squares is believed to be to a thoughtful teacher a great desideratum.

$$(a+b)^2=a^2+2ab+b^2.$$
$$(a-b)^2=a^2-2ab+b^2.$$
$$\therefore (a+b)^2-(a-b)^2=4ab,$$
$$\text{and } (a+b)^2=4ab+(a-b)^2.$$

Make $a=25$.

And $b=1, 2, 3$, etc.

Then $26^2=100+24^2$.

$27^2=200+23^2$.

$28^2=300+22^2$, etc., etc.

Evidently, then, the tens and units figures of all perfect squares of numbers from 26 to 49, inclusive, are the same as the tens and units figures of perfect squares of numbers from 24 to 1 inclusive.

Any one may now easily form the following table:

$576=24^2 \therefore 576+100=26^2=676.$
$529=23^2 \therefore 529+200=27^2=729.$
$484=22^2 \therefore 484+300=28^2=784.$
$441=21^2 \therefore 441+400=29^2=841.$
$400=20^2 \therefore 400+500=30^2=900.$
$361=19^2 \therefore 361+600=31^2=961.$
$324=18^2 \therefore 324+700=32^2=1024.$
$289=17^2 \therefore 289+800=33^2=1089.$
$256=16^2 \therefore 256+900=34^2=1156.$
$225=15^2 \therefore 225+1000=35^2=1225.$

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$196 = 14^2 \therefore 196 + 1100 = 36^2 = 1296.$
$169 = 13^2 \therefore 169 + 1200 = 37^2 = 1369.$
$144 = 12^2 \therefore 144 + 1300 = 38^2 = 1444.$
$121 = 11^2 \therefore 121 + 1400 = 39^2 = 1521.$
$100 = 10^2 \therefore 100 + 1500 = 40^2 = 1600.$
$81 = 9^2 \therefore 81 + 1600 = 41^2 = 1681.$
$64 = 8^2 \therefore 64 + 1700 = 42^2 = 1764.$
$49 = 7^2 \therefore 49 + 1800 = 43^2 = 1849.$
$36 = 6^2 \therefore 36 + 1900 = 44^2 = 1936.$
$25 = 5^2 \therefore 25 + 2000 = 45^2 = 2025.$
$16 = 4^2 \therefore 16 + 2100 = 46^2 = 2116.$
$9 = 3^2 \therefore 9 + 2200 = 47^2 = 2209.$
$4 = 2^2 \therefore 4 + 2300 = 48^2 = 2304.$
$1 = 1^2 \therefore 1 + 2400 = 49^2 = 2401.$

Hence, to determine the square of any number between 25 and 50, one has merely to find the corresponding number below 25 and augment *its* square by the number of hundreds indicated by its remoteness from 25. Thus 43 being 18 more than 25, and 7 being 18 less, the square of 43 will be $1800 + 7^2 = 1849$.

Again, since $a - b = 2a - (a + b)$, we may more conveniently obtain results by the following rule: To square any number from 25 to 50, *take the excess above* 25 *as hundreds, and augment by the square of what the number lacks of* 50.

Thus, $43^2 = (43 - 25)\cdot 100 + (50 - 43)^2 = 1849$, as above.
$47^2 = (47 - 25)\cdot 100 + 3^2 = 2209$, etc., etc.

Conversely: required, the square root of 1764. The root is plainly in the fourth decade, that is, it is between 25 and 50. The tens and units figures indicate 8. Therefore, the square root of $1764 = 50 - 8 = 42$.

Again, $\sqrt{2116} = 50 - 4 = 46$, and $\sqrt{1444} = 50 - 12 = 38$, etc., etc.

Make $a = 50$.
And $b = 1, 2, 3$, etc.
Then $51^2 = 200 + 49^2$.
$52^2 = 400 + 48^2$.
$53^2 = 600 + 47^2$,
etc., etc.

It is here further observable that the tens and units figures of perfect squares of numbers from 51 to 99 inclusive are the same as

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the tens and units figures of the squares of numbers from 49 to 1 inclusive. Now, since four times *any numbers of hundreds plus* 25, 50 or 75, gives an exact number of hundreds, it follows that the tens and units figures of the squares of numbers less than 25, represent all the possible combinations of figures in those orders of units, for *all square numbers.*

Now, as these squares give only 22 different combinations, viz.: 00, 01, 04, 09, 16, 21, 24, 25, 29, 36, 41, 44, 49, 56, 61, 64, 69, 76, 81, 84, 89, 96, it is evident that familiarity with these terminations is eminently desirable.

Remembering again, that $a - b = 2a - (a + b)$, we may formulate the rule: To square any number from 50 to 100, *take twice the excess above* 50 *as hundreds and augment by the square of what the number lacks of* 100.

Thus, $89^2 = 200\,(89 - 50) + (100 - 89)^2 = 7921$. And $93^2 = 8649$, etc.[4]

Conversely: required the square root of 3249. The root is plainly between 50 and 60. The tens and units figures indicate 7. Hence $\sqrt{3249} = 50 + 7 = 57$. And $\sqrt{3721} = 50 + 11 = 61$, etc., etc.

Note, however, that the square root of 7921 is $50 + 39$ or $100 - 11$. Now it is obviously better to make use of the latter combination. Hence, in accordance with principles already developed, if the root is in the fourth quarter of the hundred, subtract the number indicated by the tens and units figures from 100, and the difference is the required root. Thus $\sqrt{8281} = 100 - 9 = 91$, $\sqrt{9216} = 100 - 4 = 96$, $\sqrt{7569} = 100 - 13 = 87$, etc., etc.

From what we have learned already, the following are convenient and useful inferences:

To square any number from 100 to 200, *take four times the excess above* 100 *as hundreds, and augment by the square of what the number lacks of* 200. Thus $186^2 = 86 \cdot 4 \cdot 100 + 14^2 = 34596$, $178^2 = 78 \cdot 4 \cdot 100 + 22^2 = 31684$, etc., etc.

Since four times 125 is one-half of one thousand, to square any number from 125 to 250, *take one-half the excess above* 125 *as*

[4] It is well not to forget that in the case of many numbers, another plan is still more expeditious. I refer to the common scheme developed from the formula $(a + b) \cdot (a - b) = a^2 - b^2$. By transposition, this becomes $a^2 = (a + b) \cdot (a - b) + b^2$, and $93^2 = (93 + 7)(93 - 7) + 7^2 = 8649$ as before. But this plan, though commonly enough used and indispensable in its place, is not sufficiently broad for our present discussion.

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thousands, and augment by the square of what the number lacks of 250. Thus $239^2 = \frac{1}{2}(239 - 125)\cdot 1000 + 11^2 = 57121$, $246^2 = \frac{1}{2}(246 - 125)\cdot 1000 + 4^2 = 60516$, etc., etc.

Also, to square any number from 250 to 500, *take the excess above* 250 *as thousands, and augment by the square of what the number lacks of* 500. Thus, $479^2 = (479 - 250)\,1000 + 21^2 = 229441$, $467^2 = (467 - 250)\cdot 1000 + 33^2 = 218089$. It is obvious that these rules are the most useful in the vicinity of the upper limit of their scope, but when one has once rendered himself familiar with the squares of small numbers (say to 100) the ease with which succeeding steps may be taken, as indicated in the last examples, renders the more common methods almost ridiculous.

It is obvious, also, that the principle involved in these processes is universal, and that rules constructed on this plan may be multiplied *ad infinitum.* Thus, to square any number from 500 to 1000, *take twice the excess above* 500 *as thousands, and augment by the square of what the number lacks of* 1000, *e. g.:* $987^2 = 2\cdot(487)\cdot 1000 + 13^2 = 974169$.

It need not, however, be forgotten that here as in many other cases, there is a choice of methods, 987^2 being also $(987 - 13)\,1000 + 13^2 = 974169$. This, as will be observed, is only an application of the other formula, briefly discussed above, viz.: $a^2 = (a + b)(a - b) + b^2$. The utility of this formula is too often kept out of the sight of learners, until they themselves, mayhap, stumble upon it. Observe, *e. g.:*

$$
\begin{aligned}
19^2 &= 20\cdot 18 + 1.\\
28^2 &= 30\cdot 26 + 4.\\
59^2 &= 60\cdot 58 + 1.\\
97^2 &= 100\cdot 94 + 9.\\
107^2 &= 100\cdot 114 + 49.\\
983^2 &= 966\cdot 1000 + 17^2 = 966289.\\
1031^2 &= 1062\cdot 1000 + 31^2 = 1062961, \text{ etc., etc., etc.}
\end{aligned}
$$

Incidentally, this process should be made perfectly familiar. Let us now drop this branch of the subject and return to the discussion of the converse operation of evolution, ascertaining, if possible, how the principles and methods developed with numbers

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whose roots are less than 100, may be applied to numbers indefinitely larger.

Since the squares of numbers from 1 to 25 end in a series of numbers in the tens and units places, which are repeated in regular, but inverse order in the squares of numbers from 25 to 50, again, directly, in the squares of numbers from 50 to 75, and again, inversely, in the squares of numbers from 75 to 100, and so on, it is evident that what we have already proved within the limit of 100 holds good as we advance.[5]

Now, as we have already seen $\sqrt{7396} = 100 - 14 = 86$.

Similarly, $\sqrt{18769}$ is plainly between 130 and 140, *i. e.*, it is in the second quarter of the hundred. Since 69 indicates 13, the root is evidently $150 - 13 = 137$. $\sqrt{24964}$ is between 150 and 160, *i. e.*, in the third quarter of the hundred. Since 64 indicates 8, $\sqrt{24964}$ is, therefore, $150 + 8 = 158$.

In general, to read the square root of a square number, first, observe the last two figures of the power. These figures always indicate a root which is at once evident to any one who will familiarize himself with the squares of the numbers from 1 to 25.

Next, determine in which quarter of which hundred the root lies. If the root is in the first or third quarter, *add* the number indicated by the tens and units figures to the hundreds in the former case, and to the hundreds plus fifty in the latter case. If the root is in the second or fourth quarter, *subtract* the indicated number from the hundreds plus 50 in the former case and from the next greater number of hundreds in the latter case.

Thus $\sqrt{393129}$ is in the second quarter of the sixth hundred. Twenty-nine indicates 23, since $23^2 = 529$. Therefore $\sqrt{393129} = 650 - 23 = 627$. Again $\sqrt{580644}$ is in the third quarter of the seventh hundred. 44 indicates 12. Therefore $\sqrt{580644} = 750 + 12 = 762$.

The only possible difficulty remaining is to determine to which quarter of the known hundred the root of the specified power belongs. Since 24^2 and 26^2 both give 76 as tens and units figures, it is at first perhaps doubtful whether the square root of 104976 is $300 + 24$, or $350 - 24 = 326$. To obviate this pos-

[5] See remark above as to four times *any number of hundreds plus 25, 50 or 75.*

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sible difficulty, familiarity with the following table may first be acquired.

$$\begin{aligned}
&\tfrac{1}{16} = .0625.\\
\tfrac{1}{8} - &\tfrac{2}{16} = .125.\\
&\tfrac{3}{16} = .1875.\\
\tfrac{1}{4} = &\tfrac{4}{16} = .25.\\
&\tfrac{5}{16} = .3125.\\
\tfrac{3}{8} = &\tfrac{6}{16} = .375.\\
&\tfrac{7}{16} = .4375.\\
\tfrac{1}{2} = &\tfrac{8}{16} = .5.\\
&\tfrac{9}{16} = .5625.\\
\tfrac{5}{8} = &\tfrac{10}{16} = .625.\\
&\tfrac{11}{16} = .6875.\\
\tfrac{3}{4} = &\tfrac{12}{16} = .75.\\
&\tfrac{13}{16} = .8125.\\
\tfrac{7}{8} = &\tfrac{14}{16} = .875.\\
&\tfrac{15}{16} = .9375.
\end{aligned}$$

The student is here requested to observe that he may readily fall into the desirable habit of inserting or omitting decimal points at will, if thereby he can expedite his mental mathematical operations. He must also keep in mind the elementary method of squaring such binomials as $5 + \frac{1}{2}$. From an algebraic standpoint, this is obviously $5^2 + 5 \cdot \frac{1}{2} \cdot 2 + \frac{1}{4} = 6 \cdot 5 + \frac{1}{4} = 30\frac{1}{4}$. Also,

$$\begin{aligned}
(8 + \tfrac{1}{2})^2 &= 8 \cdot 8 + 8 + \tfrac{1}{4},\\
&= 9 \cdot 8 + \tfrac{1}{4},\\
&= 72\tfrac{1}{4},\\
\text{and } 12.5^2 &= 12 \cdot 13 + .25 = 156.25^{5}\\
\text{or } 125^2 &= 120 \cdot 130 + 25 = 15625, \text{ etc., etc.}
\end{aligned}$$

[5] I am reminded by this discussion of having once published an application of these principles combined, which gives at once $5^{16} = 152587890625$.

For $5^2 = 25$.
And $5^4 = 25^2 = 625$.
" $5^8 = 625^2 = 390625$.

For 625 by a point placed after the 6 becomes $6\frac{1}{4}$. Now $(6\frac{1}{4})^2 = 6 \cdot 6 + 3 + (\frac{1}{4})^2 = 39$ and 1-16th.

Writing the sixteenth decimally and removing the decimal point, we have 390625.

But $5^{16} = 390625^2 = ?$.

Restore the decimal point and $(39\ 1\text{-}16)^2 = 39^2 + 4\frac{3}{4} + (1\text{-}16)^2$. 39^2 (see above) $= 38 \cdot 40 + 1 = 1521$. $1521 + 4\frac{3}{4} = 1525\frac{3}{4}$. The square of 1-16 (.0625) if written decimally, must have the same significant figures as 625^2 just obtained, preceded by two zeros to give the correct decimal place and relation. Recollecting then that there must be eight decimal places following the 1525 and that the decimal for $\frac{3}{4}$ (.875) occupies three places, while the square of 625 must occupy the last six places, we may combine our thoughts and write without the decimal point $390625^2 = 152587890625$, which is the sixteenth power of 5 obtained mentally and by a process not extraordinarily difficult.

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We may now construct mentally a table somewhat like the following:

$125^2 = 15625$		$(1\frac{1}{4})^2 = 1\ 9\text{–}16.$
$175^2 = 30625$		$(1\frac{3}{4})^2 = 3\ 1\text{–}16.$
$225^2 = 50625$		$(2\frac{1}{4})^2 = 5\ 1\text{–}16.$
$275^2 = 75625$		$(2\frac{3}{4})^2 = 7\ 9\text{–}16.$
$325^2 = 105625$	or	$(3\frac{1}{4})^2 = 10\ 9\text{–}16.$
$375^2 = 140625$		$(3\frac{3}{4})^2 = 14\ 1\text{–}16.$
$425^2 = 180625$		$(4\frac{1}{4})^2 = 18\ 1\text{–}16.$
$475^2 = 225625$		$(4\frac{3}{4})^2 = 22\ 9\text{–}16.$
$525^2 = 275625$		$(5\frac{1}{4})^2 = 27\ 9\text{–}16.$
$575^2 = 330625$		$(5\frac{3}{4})^2 = 33\ 1\text{–}16.$

One may readily extend this table indefinitely by remembering that $(n + \frac{1}{4})^2 = n^2 + \frac{1}{2}n + \frac{1}{16}$; $(n + \frac{1}{2})^2 = n(n + 1) + \frac{1}{4}$; and $(n + \frac{3}{4})^2 = n(n + 1) + \frac{1}{2}n + \frac{9}{16}$.

It is unnecessary to discuss the suggested law of increase of squares of numbers varying by $\frac{1}{2}$ or by $\frac{1}{2}$ of 100 as here indicated, but the mnemotechnist will be sure to note that in these tables the square has the $\frac{1}{16}$ or 0625 as terminal figures for the $\frac{3}{4}$ point of the *odd* hundreds, and the $\frac{1}{4}$ point of the *even* hundreds, while the square has the $\frac{9}{16}$ or 5625 as terminal figures for the $\frac{1}{4}$ point of the *odd* hundreds, and the $\frac{3}{4}$ point of the *even* hundreds. The reason is apparent.

Another advantage of this discussion will occur to arithmeticians when in doubt as to whether a certain number is a perfect square. For example, the number 316471 cannot be a square for 71 is not in our list of the 22 and only terminations of squares. The number 172276 may or may not be a perfect square. A glance shows that if a square, the root is 424. But $424 = 400 \cdot 448 + 24^2$ which is plainly greater than the number given. Hence, 172276 is not a square number. If the doubt in any case is not so readily dissipated, the contracted method may profitably be brought into requisition.

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SECTION B.

PHYSICS.

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ADDRESS

BY

PROFESSOR H. A. ROWLAND,

VICE PRESIDENT, SECTION B.

A PLEA FOR PURE SCIENCE.[1]

THE question is sometimes asked us as to the time of year we like the best. To my mind, the spring is the most delightful; for Nature then recovers from the apathy of winter, and stirs herself to renewed life. The leaves grow, and the buds open, with a suggestion of vigor delightful to behold; and we revel in this ever-renewed life. But this cannot always last. The leaves reach their limit; the buds open to the full and pass away. Then we begin to ask ourselves whether all this display has been in vain, or whether it has led to a bountiful harvest.

So this magnificent country of ours has rivalled the vigor of spring in its growth. Forests have been levelled, and cities built and a large and powerful nation has been created on the face of the earth. We are proud of our advancement. We are proud of such cities as this, founded in a day upon a spot over which but a few years since, the red man hunted the buffalo. But we must remember that this is only the spring of our country. Our glance must not be backward; for, however beautiful leaves and blossoms are, and however marvellous their rapid increase, they are but leaves and blossoms after all. Rather should we look forward to discover what will be the outcome of all this and what the chance of harvest. For if we do this in time, we may discover the worm which threatens the ripe fruit, or the barren spot where the harvest is withering for want of water.

[1] In using the word "science," I refer to physical science, as I know nothing of natural science. Probably my remarks will, however, apply to both, but I do not know.

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I am required to address the so-called physical section of this association. Fain would I speak pleasant words to you on this subject; fain would I recount to you the progress made in this subject by my countrymen, and their noble efforts to understand the order of the universe. But I go out to gather the grain ripe to the harvest, and I find only tares. Here and there a noble head of grain rises above the weeds; but so few are they, that I find the majority of my countrymen know them not, but think that they have a waving harvest, while it is only one of weeds after all. American science is a thing of the future, and not of the present or past; and the proper course of one in my position is to consider what must be done to create a science of physics in this country, rather than to call telegraphs, electric lights, and such conveniences, by the name of science. I do not wish to underrate the value of all these things; the progress of the world depends on them, and he is to be honored who cultivates them successfully. So also the cook who invents a new and palatable dish for the table benefits the world to a certain degree; yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the *applications* of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity. I have often been asked, which was the more important to the world, pure or applied science. To have the applications of a science, the science itself must exist. Should we stop its progress, and attend only to its applications, we should soon degenerate into a people like the Chinese, who have made no progress for generations, because they have been satisfied with the applications of science, and have never sought for reasons in what they have done. The reasons constitute pure science. They have known the application of gunpowder for centuries; and yet the reasons for its peculiar action, if sought in the proper manner, would have developed the science of chemistry, and even of physics, with all their numerous applications. By contenting themselves with the fact that gunpowder will explode, and seeking no farther, they have fallen behind in the progress of the world; and we now regard this oldest and most numerous of nations as

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only barbarians, and yet our own country is in this same state. But we have done better, for we have taken the science of the old world, and applied it to all our uses, accepting it like the rain of heaven, without asking whence it came, or even acknowledging the debt of gratitude we owe to the great and unselfish workers who have given it to us; and, like the rain of heaven, this pure science has fallen upon our country, and made it great and rich and strong.

To a civilized nation of the present day, the applications of science are a necessity, and our country has hitherto succeeded in this line, only for the reason that there are certain countries in the world where pure science has been and is cultivated, and where the study of nature is considered a noble pursuit; but such countries are rare, and those who wish to pursue pure science in our own country must be prepared to face public opinion in a manner which requires much moral courage. They must be prepared to be looked down upon by every successful inventor whose shallow mind imagines that the only pursuit of mankind is wealth, and that he who obtains most has best succeeded in this world. Everybody can comprehend a million of money; but how few can comprehend any advance in scientific theory, especially in its more abstruse portions! And this, I believe, is one of the causes of the small number of persons who have ever devoted themselves to work of the higher order in any human pursuit. Man is a gregarious animal, and depends very much, for his happiness, on the sympathy of those around him; and it is rare to find one with the courage to pursue his own ideas in spite of his surroundings. In times past, men were more isolated than at present, and each came in contact with a fewer number of people. Hence that time constitutes the period when the great sculptures, paintings and poems were produced. Each man's mind was comparatively free to follow its own ideals, and the results were the great and unique works of the ancient masters. To-day the railroad and the telegraph, the books and newspapers, have united each individual man with the rest of the world: instead of his mind being an individual, a thing apart by itself, and unique, it has become so influenced by the outer world, and so dependent upon it, that it has lost its originality to a great extent. The man who in times past would naturally have been in the lowest depths of poverty, mentally and physically, to-day measures tape behind a counter, and with

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lordly air advises the naturally born genius how he may best bring his outward appearance down to a level with his own. A new idea he never had, but he can at least cover his mental nakedness with ideas imbibed from others. So the genius of the past soon perceives that his higher ideas are too high to be appreciated by the world; his mind is clipped down to the standard form; every natural offshoot upwards is repressed, until the man is no higher than his fellows. Hence the world, through the abundance of its intercourse, is reduced to a level. What was formerly a grand and magnificent landscape, with mountains ascending above the clouds, and depths whose gloom we cannot now appreciate, has become serene and peaceful. The depths have been filled, and the heights levelled, and the wavy harvests and smoky factories cover the landscape.

As far as the average man is concerned, the change is for the better. The average life of man is far pleasanter, and his mental condition better, than before. But we miss the vigor imparted by the mountains. We are tired of mediocrity, the curse of our country; we are tired of seeing our artists reduced to hirelings, and imploring congress to protect them against foreign competition; we are tired of seeing our countrymen take their science from abroad, and boast that they here convert it into wealth; we are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science, or sitting inactive while the whole world is open to investigation; lingering by the wayside while the problem of the universe remains unsolved. We wish for something higher and nobler in this country of mediocrity, for a mountain to relieve the landscape of its monotony. We are surrounded with mysteries, and have been created with minds to enjoy and reason to aid in the unfolding of such mysteries. Nature calls to us to study her, and our better feelings urge us in the same direction.

For generations there have been some few students of science who have esteemed the study of nature the most noble of pursuits. Some have been wealthy, and some poor; but they have all had one thing in common,— the love of nature and its laws. To these few men the world owes all the progress due to applied science, and yet very few ever received any payment in this world for their labors.

Faraday, the great discoverer of the principle on which all

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machines for electric lighting, electric railways, and the transmission of power, must rest, died a poor man, although others and the whole world have been enriched by his discoveries; and such must be the fate of the followers in his footsteps for some time to come.

But there will be those in the future who will study nature from pure love, and for them higher prizes than any yet obtained are waiting. We have but yet commenced our pursuit of science, and stand upon the threshold wondering what there is within. We explain the motion of the planets by the law of gravitation; but who will explain how two bodies, millions of miles apart, tend to go toward each other with a certain force? We now weigh and measure electricity and electric currents with as much ease as ordinary matter, yet have we made any approach to an explanation of the phenomenon of electricity? Light is an undulatory motion, and yet do we know what it is that undulates? Heat is motion, yet do we know what it is that moves? Ordinary matter is a common substance, and yet who shall fathom the mystery of its internal constitution?

There is room for all in the work, and the race has but commenced. The problems are not to be solved in a moment, but need the best work of the best minds, for an indefinite time.

Shall our country be contented to stand by, while other countries lead in the race? Shall we always grovel in the dust, and pick up the crumbs which fall from the rich man's table, considering ourselves richer than he because we have more crumbs, while we forget that he has the cake, which is the source of all crumbs? Shall we be swine, to whom the corn and husks are of more value than the pearls? If I read aright the signs of the times, I think we shall not always be contented with our inferior position. From looking down we have almost become blind, but may recover. In a new country, the necessities of life must be attended to first. The curse of Adam is upon us all, and we must earn our bread.

But it is the mission of applied science to render this easier for the whole world. There is a story which I once read, which will illustrate the true position of applied science in the world. A boy, more fond of reading than of work, was employed, in the early days of the steam-engine, to turn the valve at every stroke. Necessity was the mother of invention in his case: his reading was disturbed by his work, and he soon discovered that he might

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become free from his work by so tying the valve to some movable portion of the engine, as to make it move its own valve. So I consider that the true pursuit of mankind is intellectual. The scientific study of nature in all its branches, of mathematics, of mankind in its past and present, the pursuit of art, and the cultivation of all that is great and noble in the world,— these are the highest occupation of mankind. Commerce, the applications of science, the accumulation of wealth, are necessities which are a curse to those with high ideals, but a blessing to that portion of the world which has neither the ability nor the taste for higher pursuits.

As the applications of science multiply, living becomes easier, the wealth necessary for the purchase of apparatus can be obtained, and the pursuit of other things beside the necessities of life becomes possible.

But the moral qualities must also be cultivated in proportion to the wealth of the country, before much can be done in pure science. The successful sculptor or painter naturally attains to wealth through the legitimate work of his profession. The novelist, the poet, the musician, all have wealth before them as the end of a successful career. But the scientist and the mathematician have no such incentive to work: they must earn their living by other pursuits, usually teaching, and only devote their surplus time to the true pursuit of their science. And frequently, by the small salary which they receive, by the lack of instrumental and literary facilities, by the mental atmosphere in which they exist, and, most of all, by their low ideals of life, they are led to devote their surplus time to applied science or to other means of increasing their fortune. How shall we, then, honor the few, the very few, who, in spite of all difficulties, have kept their eyes fixed on the goal, and have steadily worked for pure science, giving to the world a most precious donation, which has borne fruit in our greater knowledge of the universe and in the applications to our physical life which have enriched thousands and benefited each one of us? There are also those who have every facility for the pursuit of science, who have an ample salary and every appliance for work, yet who devote themselves to commercial work, to testifying in courts of law, and to any other work to increase their present large income. Such men would be respectable if they gave up the name of professor, and took that of consulting chemist or physicist. And

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such men are needed in the community. But for a man to occupy the professor's chair in a prominent college, and, by his energy and ability in the commercial applications of his science, stand before the local community as a newspaper exponent of his science, is a disgrace both to him and his college. It is the death-blow to science in that region. Call him by his proper name, and he becomes at once a useful member of the community. Put in his place a man who shall by precept and example cultivate his science, and how different is the result! Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honorable than the accumulation of wealth. They are thus led to devote their lives to similar pursuits, and they honor the professor who has drawn them to something higher than they might otherwise have aspired to reach.

I do not wish to be misunderstood in this matter. It is no disgrace to make money by an invention, or otherwise, or to do commercial scientific work under some circumstances; but let pure science be the aim of those in the chairs of professors, and so prominently the aim that there can be no mistake. If our aim in life is wealth, let us honestly engage in commercial pursuits, and compete with others for its possession; but if we choose a life which we consider higher, let us live up to it, taking wealth or poverty as it may chance to come to us, but letting neither turn us aside from our pursuit.

The work of teaching may absorb the energies of many; and, indeed, this is the excuse given by most for not doing any scientific work. But there is an old saying, that where there is a will there is a way. Few professors do as much teaching or lecturing as the German professors, who are also noted for their elaborate papers in the scientific journals. I myself have been burdened down with work, and know what it is; and yet I here assert that all *can* find time for scientific research if they desire it. But here, again, that curse of our country, mediocrity, is upon us. Our colleges and universities seldom call for first-class men of reputation, and I have even heard the trustee of a well-known college assert that no professor should engage in research because of the time wasted. I was glad to see, soon after, by the call of a prominent scientist to that college, that the majority of the trustees did not agree with him.

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That teaching is important, goes without saying. A successful teacher is to be respected; but if he does not lead his scholars to that which is highest, is he not blameworthy? We are, then, to look to the colleges and universities of the land for most of the work in pure science which is done. Let us therefore examine these latter, and see what the prospect is.

One, whom perhaps we may here style a practical follower of Ruskin, has stated that while in this country he was variously designated by the title of captain, colonel, and professor. The story may or may not be true, but we all know enough of the customs of our countrymen not to dispute it on general principles. All men are born equal: some men are captains, colonels, and professors, and therefore all men are such. The logic is conclusive; and the same kind of logic seems to have been applied to our schools, colleges, and universities. I have before me the report of the commissioner of education for 1880. According to that report, there were 389,[1] or say, in round numbers, 400 institutions, calling themselves colleges or universities, in our country! We may well exclaim that ours is a great country, having more than the whole world beside. The fact is sufficient. The whole earth could hardly support such a number of first-class institutions. The curse of mediocrity must be upon them, to swarm in such numbers. They must be a cloud of mosquitoes, instead of eagles as they profess; and this becomes evident on further analysis. About one-third aspire to the name of university; and I note one called by that name which has two professors and eighteen students, and another having three teachers and twelve students! These instances are not unique, for the number of small institutions and schools which call themselves universities is very great. It is difficult to decide from the statistics alone the exact standing of these institutions. The extremes are easy to manage. Who can doubt that an institution with over eight hundred students, and a faculty of seventy, is of a higher grade than those above cited having ten or twenty students and two or three in the faculty? Yet this is not always true; for I note one institution with over five hundred students which is known to me personally as of the grade of a high school. The statistics are more or less defective, and it would much weaken the force of my remarks if I went too

[1] 364 reported on, and 25 not reported.

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much into detail. I append the following tables, however, of 330 so-called colleges and universities:—

218	had from	0	to	100	students.
88	" "	100	"	200	"
12	" "	200	"	300	"
6	" "	300	"	500	"
6	over	500			

Of 322 so-called colleges and universities:—

206	had	0	to 10	in the faculty.
99	"	10	" 20	" "
17	"	20	or over	" "

If the statistics were forthcoming,—and possibly they may exist,—we might also get an idea of the standing of these institutions and their approach to the true university idea, by the average age of the scholars. Possibly also the ratio of number of scholars to teachers might be of some help. All these methods give an approximation to the present standing of the institutions. But there is another method of attacking the problem, which is very exact, yet it only gives us the *possibilities* in the case of the institution. I refer to the wealth of the institution. In estimating the wealth, I have not included the value of grounds and building, for this is of little importance, either to the present or future standing of the institution, as good work can be done in a hovel as in a palace. I have taken the productive funds of the institution as the basis of estimate. I find:—

234	have	below	$500,000.
8	"	between	$500,000 and $1,000,000.
8	"	over	$1,000,000.

There is no fact more firmly established, all over the world, than that the higher education can never be made to pay for itself. Usually the cost to a college, of educating a young man, very much exceeds what he pays for it, and is often three or four times as much. The higher the education, the greater this proportion will be; and a university of the highest class should anticipate only a small accession to its income from the fees of students. Hence the test I have applied must give a true representation of the possibilities in every case. According to the figures, only six-

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teen colleges and universities have $500,000 or over of invested funds, and only one-half of these have $1,000,000 and over. Now, even the latter sum is a very small endowment for a college; and to call any institution a university which has less than $1,000,000 is to render it absurd in the face of the world. And yet more than 100 of our institutions, many of them very respectable colleges, have abused the word "university" in this manner. It is to be hoped that the endowment of the more respectable of these institutions may be increased, as many of them deserve it; and their unfortunate appellation has probably been repented of long since.

But what shall we think of a community that gives the charter of a university to an institution with a total of $20,000 endowment, two so-called professors, and eighteen students! or another with three professors, twelve students, and a total of $27,000 endowment, mostly invested in buildings! And yet there are very many similar institutions; there being sixteen with three professors or less, and very many indeed with only four or five.

Such facts as these could only exist in a democratic country, where pride is taken in reducing everything to a level. And I may also say, that it can only exist in the early days of such a democracy; for an intelligent public will soon perceive that calling a thing by a wrong name does not change its character, and that truth, above all things, should be taught to the youth of the nation.

It may be urged, that all these institutions are doing good work in education; and that many young men are thus taught, who could not afford to go to a true college or university. But I do not object to the education,— though I have no doubt an investigation would disclose equal absurdities here,— for it is aside from my object. But I do object to lowering the ideals of the youth of the country. Let them know that they are attending a school, and not a university; and let them know that above them comes the college, and above that the university. Let them be taught that they are only half educated, and that there are persons in the world by whose side they are but atoms. In other words, let them be taught the truth.

It may be that some small institutions are of high grade, especially those which are new; but who can doubt that more than two-thirds of our institutions calling themselves colleges and universities are unworthy of the name? Each one of these institutions

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has so-called professors, but it is evident that they can be only of the grade of teachers. Why should they not be so called? The position of teacher is an honored one, but is not made more honorable by the assumption of a false title. Furthermore, the multiplication of the title, and the ease with which it can be obtained, render it scarcely worth striving for. When the man of energy, ability, and perhaps genius is rewarded by the same title and emoluments as the commonplace man with the modicum of knowledge, who takes to teaching, not because of any aptitude for his work, but possibly because he has not the energy to compete with his fellow-men in business, then I say one of the inducements for first-class men to become professors is gone.

When work and ability are required for the position, and when the professor is expected to keep up with the progress of his subject, and to do all in his power to advance it, and when he is selected for these reasons, then the position will be worth working for, and the successful competitor will be honored accordingly. The chivalric spirit which prompted Faraday to devote his life to the study of nature may actuate a few noble men to give their lives to scientific work; but if we wish to cultivate this highest class of men in science, we must open a career for them worthy of their efforts.

Jenny Lind, with her beautiful voice, would have cultivated it to some extent in her native village; yet who would expect her to travel over the world, and give concerts for nothing? and how would she have been able to do so if she had wished? And so the scientific man, whatever his natural talents, must have instruments and a library, and a suitable and respectable salary to live upon, before he is able to exert himself to his full capacity. This is true of advance in all the higher departments of human learning, and yet something more is necessary. It is not those in this country who receive the largest salary, and have positions in the richest colleges, who have advanced their subject the most: men receiving the highest salaries, and occupying the professor's chair, are to-day doing absolutely nothing in pure science, but are striving by the commercial applications of their science to increase their already large salary. Such pursuits, as I have said before, are honorable in their proper place; but the duty of a professor is to advance his science, and to set an example of pure and true devotion to it which shall demonstrate to his students and the

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world that there is something high and noble worth living for. Money-changers are often respectable men, and yet they were once severely rebuked for carrying on their trade in the court of the temple.

Wealth does not constitute a university, buildings do not: it is the men who constitute its faculty, and the students who learn from them. It is the last and highest step which the mere student takes. He goes forth into the world, and the height to which he rises has been influenced by the ideals which he has consciously or unconsciously imbibed in his university. If the professors under whom he has studied have been high in their profession, and have themselves had high ideals; if they have considered the advance of their particular subject their highest work in life, and are themselves honored for their intellect throughout the world,— the student is drawn toward that which is highest, and ever after in life has high ideals. But if the student is taught by what are sometimes called good teachers, and teachers only, who know little more than the student, and who are often surpassed and even despised by him, no one can doubt the lowered tone of his mind. He finds that by his feeble efforts he can surpass one to whom a university has given its highest honor; and he begins to think that he himself is a born genius, and the incentive to work is gone. He is great by the side of the molehill, and does not know any mountain to compare himself with.

A university should have not only great men in its faculty, but have numerous minor professors and assistants of all kinds, and should encourage the highest work, if for no other reason than to encourage the student to his highest efforts.

But, assuming that the professor has high ideals, wealth such as only a large and high university can command is necessary to allow him the fullest development.

And this is specially so in our science of physics. In the early days of physics and chemistry, many of the fundamental experiments could be performed with the simplest apparatus. And so we often find the names of Wollaston and Faraday mentioned as needing scarcely anything for their researches. Much can even now be done with the simplest apparatus, and nobody, except the utterly incompetent, need stop for want of it; but the fact remains, that one can only be free to investigate in all departments of chemistry and physics, when he not only has a complete labo-

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ratory at his command, but a fund to draw on for the expenses of each experiment. That simplest of the departments of physics, namely, astronomy, has now reached such perfection that nobody can expect to do much more in it without a perfectly equipped observatory; and even this would be useless without an income sufficient to employ a corps of assistants to make the observations and computations. But even in this simplest of physical subjects, there is great misunderstanding. Our country has very many excellent observatories, and yet little work is done in comparison, because no provision has been made for maintaining the work of the observatory; and the wealth which, if concentrated, might have made one effective observatory which would prove a benefit to astronomical science, when scattered among a half-dozen merely furnishes telescopes for the people in the surrounding region to view the moon with. And here I strike the keynote of at least one need of our country, if she would stand well in science; and the following item which I clip from a newspaper will illustrate the matter:—

"The eccentric old Canadian, Arunah Huntington, who left $200,000 to be divided among the public schools of Vermont, has done something which will be of little practical value to the schools. Each district will be entitled to the insignificant sum of $10, which will not advance much the cause of education."

Nobody will dispute the folly of such a bequest, or the folly of filling the country with telescopes to look at the moon, and calling them observatories. How much better to concentrate the wealth into a few parcels, and make first-class observatories and institutions with it!

Is it possible that any of our four hundred colleges and universities have love enough of learning to unite with each other and form larger institutions? Is it possible that any have such a love of truth that they are willing to be called by their right name? I fear not; for the spirit of expectation, which is analogous to the spirit of gambling, is strong in the American breast, and each institution which now, except in name, slumbers in obscurity, expects in time to bloom out into full prosperity. Although many of them are under religious influence, where truth is inculcated, and where men are taught to take a low seat at the table in order that they may be honored by being called up higher, and not dishonored by being thrust down lower, yet these institutions have

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thrust themselves into the highest seats, and cannot probably be dislodged.

But would it not be possible so to change public opinion that no college could be founded with a less endowment than say $1,000,000, or no university with less than three or four times that amount? From the report of the commissioner of education, I learn that such a thing is taking place; that the tendency towards large institutions is increasing, and that it is principally in the west and southwest that the multiplication of small institutions with big names is to be feared most, and that the east is almost ready for the great coming university.

The total wealth of the four hundred colleges and universities in 1880 was about $40,000,000 in buildings, and $43,000,000 in productive funds. This would be sufficient for one great university of $10,000,000, four of $5,000,000, and twenty-six colleges of $2,000,000 each. But such an idea can of course never be carried out. Government appropriations are out of the question, because no political trickery must be allowed around the ideal institution.

In the year 1880 the private bequests to all schools and colleges amounted to about $5,500,000; and, although there was one bequest of $1,250,000, yet the amount does not appear to be phenomenal. It would thus seem that the total amount was about five million dollars in one year, of which more than half is given to so-called colleges and universities. It would be very difficult to regulate these bequests so that they might be concentrated sufficiently to produce an immediate result. But the figures show that generosity is a prominent feature of the American people, and that the needs of the country only have to be appreciated to have the funds forthcoming. We must make the need of research and of pure science felt in the country. We must live such lives of pure devotion to our science, that all shall see that we ask for money, not that we may live in indolent ease at the expense of charity, but that we may work for that which has advanced and will advance the world more than any other subject, both intellectually and physically. We must live such lives as to neutralize the influence of those who in high places have degraded their profession, or have given themselves over to ease, and do nothing for the science which they represent. Let us do what we can with the present means at our disposal. There is not one of us who is situated in the position best adapted to bring out all his powers, and to allow him to do most for his science.

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All have their difficulties, and I do not think that circumstances will ever radically change a man. If a man has the instinct of research in him, it will always show itself in some form. But circumstances may direct it into new paths, or may foster it so that what would otherwise have died as a bud now blossoms and ripens into the perfect fruit.

Americans have shown no lack of invention in small things; and the same spirit when united to knowledge and love of science, becomes the spirit of research. The telegraph-operator, with his limited knowledge of electricity and its laws, naturally turns his attention to the improvement of the only electrical instrument he knows anything about; and his researches would be confined to the limited sphere of his knowledge, and to the simple laws with which he is acquainted. But as his knowledge increases, and the field broadens before him, as he studies the mathematical theory of the subject, and the electro-magnetic theory of light loses the dim haze due to distance, and becomes his constant companion, the telegraph instrument becomes to him a toy, and his effort to discover something new becomes research in pure science.

It is useless to attempt to advance science until one has mastered the science: he must step to the front before his blows can tell in the strife. Furthermore, I do not believe anybody can be thorough in any department of science, without wishing to advance it. In the study of what is known, in the reading of the scientific journals, and the discussions therein contained of the current scientific questions, one would obtain an impulse to work, even though it did not before exist; and the same spirit which prompted him to seek what was already known would make him wish to know the unknown. And I may say that I never met a case of thorough knowledge in my own science, except in the case of well-known investigators. I have met men who talked well, and I have sometimes asked myself why they did not do something; but further knowledge of their character has shown the superficiality of their knowledge. I am no longer a believer in men who could do something if they would, or would do something if they had a chance. They are impostors. If the spirit is there, it will show itself in spite of circumstances.

As I remarked before, the investigator in pure science is usually a professor. He must teach as well as investigate. It is a question which has been discussed in late years, as to whether these

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two functions had better be combined in the same individual, or separated. It seems to be the opinion of most, that a certain amount of teaching is conducive, rather than otherwise, to the spirit of research. I myself think that this is true, and I should myself not like to give up my daily lecture; but one must not be overburdened. I suppose that the true solution, in many cases, would be found in the multiplication of assistants, not only for the work of teaching but of research. Some men are gifted with more ideas than they can work out with their own hands, and the world is losing much by not supplying them with extra hands. Life is short: old age comes quickly, and the amount one pair of hands can do is very limited. What sort of shop would that be, or what sort of factory, where one man had to do all the work with his own hands? It is a fact in nature, which no democracy can change, that men are *not* equal,—that some have brains, and some hands; and no idle talk about equality can ever subvert the order of the universe.

I know of no institution in this country where assistants are supplied to aid directly in research; yet why should it not be so? Even the absence of assistant professors and assistants of all kinds, to aid in teaching, is very noticeable, and must be remedied before we can expect much.

There are many physical problems, especially those requiring exact measurements, which cannot be carried out by one man, and can only be successfully attacked by the most elaborate apparatus, and with a full corps of assistants. Such are Regnault's experiments on the fundamental laws of gases and vapors, made thirty or forty years ago by aid from the French government, and which are the standards to this day. Although these experiments were made with a view to the practical calculation of the steam-engine, yet they were carried out in such a broad spirit that they have been of the greatest theoretical use. Again, what would astronomy have done without the endowment of observatories? By their means, that science has become the most perfect of all branches of physics, as it should be from its simplicity. There is no doubt, in my mind, that similar institutions for other branches of physics, or, better, to include the whole of physics, would be equally successful. A large and perfectly equipped physical laboratory with its large revenues, its corps of professors and assistants, and its machine-shop for the construction of new apparatus, would be able

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to advance our science quite as much as endowed observatories have advanced astronomy. But such a laboratory should not be founded rashly. The value will depend entirely on the physicist at its head, who has to devise the plan, and to start it into practical working. Such a man will always be rare, and cannot always be obtained. After one had been successfully started, others could follow; for imitation requires little brains.

One could not be certain of getting the proper man every time, but the means of appointment should be most carefully studied so as to secure a good average. There can be no doubt that the appointment should rest with a scientific body capable of judging the highest work of each candidate.

Should any popular element enter, the person chosen would be of the literary-scientific order, or the dabbler on the outskirts who presents his small discoveries in the most theatrical manner. What is required is a man of depth, who has such an insight into physical science that he can tell when blows will best tell for its advancement.

Such a grand laboratory as I describe does not exist in the world, at present, for the study of physics. But no trouble has ever been found in obtaining means to endow astronomical science. Everybody can appreciate, to some extent, the value of an observatory; as astronomy is the simplest of scientific subjects, and has very quickly reached a position where elaborate instruments and costly computations are necessary to further advance. The whole domain of physics is so wide that workers have hitherto found enough to do. But it cannot always be so, and the time has even now arrived when such a grand laboratory should be founded. Shall our country take the lead in this matter, or shall we wait for foreign countries to go before? They will be built in the future, but when and how is the question.

Several institutions are now putting up laboratories for physics. They are mostly for teaching, and we can expect only a comparatively small amount of work from most of them. But they show progress; and, if the progress be as quick in this direction as in others, we should be able to see a great change before the end of our lives.

As stated before, men are influenced by the sympathy of those with whom they come in contact. It is impossible to change public opinion in our favor immediately; and, indeed, we must always

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seek to lead it, and not be guided by it. For pure science is the pioneer who must not hover about cities and civilized countries, but must strike into unknown forests, and climb the hitherto inaccessible mountains which lead to and command a view of the promised land,—the land which science promises us in the future; which shall not only flow with milk and honey, but shall give us a better and more glorious idea of this wonderful universe. We must create a public opinion in our favor, but it need not at first be the general public. We must be contented to stand aside, and see the honors of the world for a time given to our inferiors; and must be better contented with the approval of our own consciences, and of the very few who are capable of judging our work, than of the whole world beside. Let us look to the other physicists, not in our own town, not in our own country, but in the whole world, for the words of praise which are to encourage us, or the words of blame which are to stimulate us to renewed effort. For what to us is the praise of the ignorant? Let us join together in the bonds of our scientific societies, and encourage each other, as we are now doing, in the pursuit of our favorite study; knowing that the world will sometime recognize our services, and knowing, also, that we constitute the most important element in human progress.

But danger is also near, even in our societies. When the average tone of the society is low, when the highest honors are given to the mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind, a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill, and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavor to grow.

We cannot all be men of genius; but we can, at least, point them out to those around us. We may not be able to benefit science much ourselves; but we can have high ideals on the subject, and instil them into those with whom we come in contact.

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For the good of ourselves, for the good of our country, for the good to the world, it is incumbent on us to form a true estimate of the worth and standing of persons and things, and to set before our own minds all that is great and good and noble, all that is most important for scientific advance, above the mean and low and unimportant.

It is very often said, that a man has a right to his opinion. This might be true for a man on a desert island, whose error would influence only himself; but when he opens his lips to instruct others, or even when he signifies his opinions by his daily life, then he is directly responsible for all his errors of judgment or fact. He has no right to think a molehill as big as a mountain, nor to teach it, any more than he has to think the world is flat, and teach that it is so. The facts and laws of our science have *not* equal importance, neither have the men who cultivate the science achieved equal results. One thing is greater than another, and we have no right to neglect the order. Thus shall our minds be guided aright, and our efforts be toward that which is the highest.

Then shall we see that no physicist of the first class has ever existed in this country, that we must look to other countries for our leaders in that subject, and that the few excellent workers in our country must receive many accessions from without before they can constitute an American science, or do their share in the world's work.

But let me return to the subject of scientific societies. Here American science has its hardest problem to contend with. There are very many local societies dignified by high-sounding names, each having its local celebrity, to whom the privilege of describing some crab with an extra claw, which he found in his morning ramble, is inestimable. And there are some academies of science, situated at our seats of learning, which are doing good work in their locality. But distances are so great that it is difficult to collect men together at any one point. The American Association, which we are now attending, is not a scientific academy, and does not profess to be more than a gathering of all who are interested in science, to read papers and enjoy social intercourse. The National Academy of Sciences contains eminent men from the whole country, but then it is only for the purpose of advising the government freely on scientific matters. It has no building, it has

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no library; and it publishes nothing except the information which it freely gives to the government, which does nothing for it in return. It has not had much effect directly on American science; but the liberality of the government in the way of scientific expeditions, publications, etc., is at least partly due to its influence, and in this way it has done much good. But it in no way takes the place of the great Royal society, or the great academies of science at Paris, Berlin, Vienna, St. Petersburgh, Munich, and, indeed, all the European capitals and large cities. These, by their publications, give to the young student, as well as to the more advanced physicist, models of all that is considered excellent; and to become a member is one of the highest honors to which he can aspire, while to write a memoir which the academy considers worthy to be published in its transactions excites each one to his highest effort.

The American Academy of Sciences in Boston is perhaps our nearest representation of this class of academies, but its limitation of membership to the state deprives it of a national character.

But there is another matter which influences the growth of our science.

As it is necessary for us still to look abroad for our highest inspiration in pure science, and as science is not an affair of one town or one country, but of the whole world, it becomes us all to read the current journals of science and the great transactions of foreign societies, as well as those of our own countries. These great transactions and journals should be in the library of every institution of learning in the country, where science is taught. How can teachers and professors be expected to know what has been discovered in the past, or is being discovered now, if these are not provided? Has any institution a right to starve mentally the teachers whom it employs, or the students who come to it? There can be but one answer to this; and an institution calling itself a university, and not having the current scientific journals upon its table or the transactions of societies upon its library-shelves, is certainly not doing its best to cultivate all that is best in this world.

We call this a free country, and yet it is the only one where there is a direct tax upon the pursuit of science. The low state of pure science in our country may possibly be attributed to the youth of the country; but a direct tax, to prevent the growth of

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our country in that subject, cannot be looked upon as other than a deep disgrace. I refer to the duty upon foreign books and periodicals. In our science, no books above elementary ones have ever been published, or are likely to be published in this country; and yet every teacher in physics must have them, not only in the college library, but on his own shelves, and must pay the government of this country to allow him to use a portion of his small salary to buy that which is to do good to the whole country. All freedom of intercourse which is necessary to foster our growing science is thus broken off, and that which might, in time, relieve our country of its mediocrity, is nipped in the bud by our government, which is most liberal when appealed to directly on scientific subjects.

One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off. Our scientific associations and societies must make themselves heard in this matter, and show those in authority how the matter stands.

In conclusion, let me say once more, that I do not believe that our country is to remain long in its present position. The science of physics, in whose applications our country glories, is to arise among us, and make us respected by the nations of the world. Such a prophecy may seem rash with regard to a nation which does not yet do enough physical work to support a physical journal. But we know the speed with which we advance in this country: we see cities springing up in a night, and other wonders performed at an unprecedented rate. And now we see physical laboratories being built, we see a great demand for thoroughly trained physicists, who have not shirked their mathematics, both as professors and in so-called practical life; and perhaps we have the feeling, common to all true Americans, that our country is going forward to a glorious future, when we shall lead the world in the strife for intellectual prizes as we now do in the strife for wealth.

But if this is to be so, we must not aim low. The problems of the universe cannot be solved without labor; they cannot be attacked without the proper intellectual as well as physical tools; and no physicist need expect to go far without his mathematics. No one expects a horse to win in a great and long race who has not been properly trained; and it would be folly to attempt to win

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with one, however pure his blood and high his pedigree, without it. The problems we solve are more difficult than any race; the highest intellect cannot hope to succeed without proper preparation. The great prizes are reserved for the greatest efforts of the greatest intellects, who have kept their mental eye bright and flesh hard by constant exercise. Apparatus can be bought with money, talents may come to us at birth; but our mental tools, our mathematics, our experimental ability, our knowledge of what others have done before us, all have to be obtained by work. The time is almost past, even in our own country, when third-rate men can find a place as teachers, because they are unfit for everything else. We wish to see brains and learning, combined with energy and immense working power, in the professor's chair; but, above all, we wish to see that high and chivalrous spirit which causes one to pursue his idea in spite of all difficulties, to work at the problems of nature with the approval of his own conscience, and not of men before him. Let him fit himself for the struggle with all the weapons which mathematics and the experience of those gone before him can furnish, and let him enter the arena with the fixed and stern purpose to conquer. Let him not be contented to stand back with the crowd of mediocrity, but let him press forward for a front place in the strife.

The whole universe is before us to study. The greatest labor of the greatest minds has only given us a few pearls; and yet the limitless ocean, with its hidden depths filled with diamonds and precious stones, is before us. The problem of the universe is yet unsolved, and the mystery involved in one single atom yet eludes us. The field of research only opens wider and wider as we advance, and our minds are lost in wonder and astonishment at the grandeur and beauty unfolded before us. Shall we help in this grand work, or not? Shall our country do its share, or shall it still live in the almshouse of the world?

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PAPERS READ.

Determination of the relation between the Imperial Yard and the Metre of the Archives. By **William A. Rogers** of Cambridge, Mass.

[ABSTRACT.]

This paper was a continuation of one upon the same subject presented at the Montreal meeting. The mean result of the determinations up to that time was as follows: Imperial Yard + 3.37015 inches = Metre des Archives.

The writer stated at that time, that he should not like to be held to a very strict account with regard to the last decimal figure, or even the last two decimal figures, on account of the difficulty of obtaining the requisite data.

Since the meeting last year, additional data have been obtained. In February of the present year, a combined yard and metre was received from Paris. The yard was compared with the Imperial Yard, in 1880, by Mr. Chaney, the Warden of the Imperial Standards. During the interval between 1880 and February of the present year, this metre has received repeated comparisons with the metre of the International Bureau, under the direction of Dr. Pernet. According to his report, this metre is 310 mikrons too short at 0° centigrade; for the same temperature, the yard was found by Mr. Chaney to be 20.7 mikrons too short.

Comparing the metre and the yard upon this bar with the bronze yard and metre described at Montreal, and combining the results with those previously found, the relation was found as follows: Imperial Yard + 3.37027 inches = Metre des Archives.

The details of the investigation will be found in the Proceedings of the American Academy of Arts and Sciences for 1883.

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THE STATIC TELEPHONE. By A. E. DOLBEAR of College Hill, Mass.

[ABSTRACT.]

IN the static telephone, a ring of hard rubber is used within which are two parallel metal-plates separated by a body of air (a non-conductor), one plate connected with the line from the transmitter. The electrifying of one plate then causes attraction or repulsion of the free plate, and thus a sound in the receiver. This does away with magnetism. This system therefore requires a very large electro-motive force, and uses an induction coil of 2,000 ohms. A ground or return circuit is not needed here. The equivalent is the body itself. There is no passage of electricity from plate to plate: the action is purely inductive through space. The insulation is accomplished by the intervening air space, and by a coating of varnish, — an excellent di-electric. There is a device to charge the induction plates in connection with this instrument, which keeps it constantly up to its full possibilities. When this instrument is fully charged, and the electrical conditions are perfect, the receiver may be entirely disconnected from the transmitter, and sounds and conversation can still be heard, even across a room.

He also called attention to the fact, that instruments that have been in use work much better than new ones, as each plate acts as a condenser.[1]

NEW FORM OF SELENIUM CELL, WITH SOME REMARKABLE ELECTRICAL DISCOVERIES MADE BY ITS USE. By CHARLES E. FRITTS of New York, N. Y.

[ABSTRACT.]

THE author has invented an entirely new form of cell, having the following novel features: 1. Its resistance can be made as low as desired. Some cells have been made having a resistance as low as 9 ohms in the dark. 2. They are far more sensitive to light

[1] The discussion following the reading of this paper is reported in SCIENCE of Aug. 24, p. 285.

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than any before known. One was cited which had fifteen times as much resistance in dark as in diffused daylight. 3. They show the other properties of selenium in a degree equally remarkable. 4. The light is caused to strike the selenium in the same plane or general direction as the current. 5. Their construction is such that they are easily handled and used, and form a practical working apparatus, both for scientific researches and technical uses. The cost when completed is about $100 each.

By experiments he has discovered: 1. That, by simply reversing the direction of a current through the cell, its resistance will become in some cases as much as ten or fifteen times as high as before. When the original direction of current is restored, its resistance falls again. This effect can be repeated any number of times. It is sensitive to light in both cases, but is generally more sensitive with the current entering the selenium at the same surface which the light is acting upon.

2. By changing the electromotive force of the current through the cell, its resistance is changed enormously. By substituting 23 Leclanché elements in place of 1, the resistance of a cell was in one case changed from 14,000 ohms to 4,600 ohms. On lowering the battery power the reverse effect is produced. This change can be produced as often as desired.

3. Very moderate changes of temperature (say 10° to 50° Fahr.) can change the resistance of a cell some hundreds or thousands of ohms in a few seconds. This change is temporary. The sensitiveness to light is greater when the cell is cold.

4. An intermittent current will produce the same effect. In one case it lowered the resistance of a cell from 50,000 ohms to 33,000 ohms almost instantly. But this change was permanent.

Mr. Fritts has produced a colorless transparent form of selenium, by subjecting pure selenium to high heat under pressure. It appears to be a better conductor even than the ordinary granular or crystalline form of selenium.

Other interesting discoveries were described, and the attention of physicists was called to these phenomena, as being worthy of careful study, and promising discoveries of the highest importance, both theoretically and practically, in return for properly conducted investigations.

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The Magnetophone, or the modification of the Magnetic Field by the rotation of a perforated Metallic Disk.

By H. S. Carhart of Evanston, Ill.

[ABSTRACT.[1]]

The experiments of Bell, Preece, and others, on the radiophone, suggested the possibility of interrupting, or at least periodically modifying, the lines of force proceeding from the poles of a magnet, by means of a disk of sheet-iron, perforated with a series of equidistant holes, and rotated so that the holes should pass directly in front of the magnetic pole. It is well known that the armature placed on the poles of a permanent magnet diminishes the strength of the external field of force by furnishing superior facilities for the formation of polarized chains of particles from pole to pole. This is the case even when the armature does not touch the poles, but is in close proximity to them.

If a piece of sheet-iron be placed over the poles of a magnet without touching, and magnetic curves be developed on paper above the iron, they will be found to exhibit less intense and less sharply defined magnetic action than when the sheet-iron is removed. If, however, a small hole be drilled directly over each magnetic pole, the screening action of the sheet-iron is modified in much the same way as when a hole is made in a screen opaque to light; for the developed curves show distinctly the outline of the holes. If, therefore, the sheet-iron in the form of a circular plate, pierced with a number of holes, be rapidly rotated between the pole of a magnet and a small induction bobbin, the action of the magnet on the core of the bobbin will be periodically modified, because of the passing holes; and hence induced currents will flow through a circuit including the bobbin. A disk of sheet-iron was pierced with two circles of quarter-inch holes concentric with the disk, the number of holes in the two circles being thirty-two and sixty-four respectively. On one side of the disk was placed a horseshoe magnet with its poles very near the rows of holes; on the other side were arranged two corresponding induction bobbins. The circuit was completed through a telephone and either bobbin at pleasure. Upon rotating the disk rapidly, a clear

[1] This paper is published in full with illustrations in SCIENCE for September 21, 1883.

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musical sound was produced in the telephone, the pitch rising with the rapidity of rotation. Moreover, the bobbin opposite the circle of sixty-four holes gave the octave above the other, and each gave a note of the same pitch as was produced by blowing a stream of air through the corresponding holes.

Non-magnetic metallic disks produce similar musical notes by the periodic modification of the magnetic field by means of the distortion or bending of the lines of force. A periodic movement of the lines of force will take place when the disk rotates, because the solid parts deflect the lines in the direction of the rotation, while the passage of a hole allows them to fall back.

MAGNETIC SURVEY OF MISSOURI. By F. E. NIPHER of St. Louis, Missouri.

[ABSTRACT.]

IN the spring of 1878 a survey of Missouri was begun, which was expected to determine all points in regard to terrestrial magnetism: 160 points have been covered. The work was undertaken under private auspices, most of the money tendered unasked, and the work has been carried on successfully until the present time. The first three years were spent in making a preliminary survey. In the early part of the survey we labored under great difficulties, because I supposed that the lines of equal value, laid down upon the observations given in the coast-survey charts, were substantially correct; so that time was frequently lost in repeating values at stations left behind, in order to be certain that no error had been committed. But when we settled down to the conclusion that we really knew nothing about the matter, we had very much less trouble. At first, intensity determinations were made at each station; but in later years, since the magnets have proved so satisfactory, the plan was adopted of making absolute determinations only at regular intervals during the summer. The temperature corrections for the magnet were made twice,— once in 1878, and once two years ago,— and they agreed very closely with each other.

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The dip circle was a large one, such as was formerly much used, and which was found to be an excellent instrument, though rather clumsy to carry. The charts which have been prepared show what the results were. In a former communication to the association at Cincinnati, I suggested an explanation of the peculiar flexures of the isogonic lines, as being due to earth-currents which seemed to be deflected by the moist river-valleys. The map upon which that hypothesis was based represented observations taken over the entire state. By re-determination we have found that those observations were all correct; but more detailed work shows that this explanation is not admissible. There is no evidence of the fact that contour has anything to do with the deviation of the needle from the normal values. Similar flexures are also seen in the lines of equal inclination and the lines of intensity. One and perhaps two years will be required to accomplish the work properly. There is nothing new in the subject, except the rather unexpected flexures which we found in these lines. It shows very clearly that the isogonic lines which are published for the use of surveyors are of no earthly use. Work ought to be done in a detailed way over the whole country; and I hope we shall some time be able to combine with these determinations a series of magnetic values at ten or twelve different stations in the state of Missouri, and also simultaneous determinations of earth-currents upon lines making angles with each other at the different stations. Similar variations would probably be found in the states of Illinois and Iowa.

A Method of distributing Weather Forecasts by means of Railways. By T. C. Mendenhall of Columbus, Ohio.

[ABSTRACT.]

This system has only been in operation in Ohio for about a year. To distribute forecasts, we place signals upon the sides of the baggage-cars, as distinct as possible from each other, so as to be easily recognized at considerable distances, and also to convey as much meaning as possible, so as to predict as many different conditions. We adopted a combination of form and color. The

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signals are three in number as to form, and two in number as to color. The red signals are confined to predictions as to temperature,— rise in temperature, stationary temperature, falling temperature. The other color is blue, and that is confined to predictions in regard to the general state of the weather. The question of form was a good deal considered, and three forms were adopted. We adopted the sun, moon, and star, because everybody was familiar with those words. We experimented with the triangle, and finally rejected it. The device for attaching to the car is due to Mr. Anderson, who has been in the service of the board of commissioners for the past year, and it is a really happy device. The signal is made as large as possible, and the disk can be seen a long distance. The red sun and blue moon mean higher temperature and general rain. The crescent means lower temperature; the full disk of blue means general rain; the star represents local rains. With regard to the proper working of the system, though it has been in operation but a short time, it has really done good work. We receive special telegrams every morning, and they are transmitted to the train-despatchers at five o'clock. We are as yet operating it only on one railroad. It happens, fortunately, that that road goes through an agricultural region of considerable importance. It is the road connecting the cities of Columbus and Cleveland. Two trains start out in the morning, at the middle point between those cities. The signals are put on the cars at five o'clock in the morning; and as they run through the morning hours, the farmers along the line can have an opportunity of seeing them, and predicting the weather for the day. The railway company circulated through the whole line little cards, having these signals displayed in colors, with their meaning in every combination. This helps us, because it enables everybody to understand what is meant. A recent communication from Gen. Hazen indicates a disposition on the part of the general government to take hold of the matter, and bring it into general operation as far as possible. Postal-cards have been sent to various persons along the line, with questions in regard to the practical working of the system, which are answered and sent in at the end of every week; and we find, that, on the average, eighty per cent of the predictions are verified.

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PLAN FOR A STATE WEATHER SERVICE. By F. E. NIPHER of St. Louis, Mo.

[ABSTRACT.]

WHILE a good many are accommodated by the weather-signals which Professor Mendenhall has already inaugurated, many live a distance from the railroad, and cannot be interested in a scheme which makes it necessary to travel eight or ten miles to learn about the weather, because they might be interested in a different kind of weather by the time they got home. The information might be most easily circulated by telegraphing from picket-stations to the westward. There might be a line of stations on the railroad north and south; and stations might be found necessary in Nebraska, which would give immediate warning to the central office whenever it began to rain at the station; and a code might be arranged, so as to give the idea of the operator as to the probable violence or duration of the rain. Of course it would be necessary to make special study of the general laws for the progress of summer rains. Supposing the information is telegraphed to the central station, the predictions can easily be made out as soon as the picket-stations could be reached, and a clear idea obtained as to the probable direction of the storm, and the time at which it would reach the different portions of the state. That information could be transmitted by the railway companies. Finally, we should make more intimate connection between these and private telegraph-lines which can be constructed by the persons who are to be served with the weather signals. This plan contemplates the erection of private telegraph-lines leading in from the country to the stations. Upon a twenty-mile line, which would be a frequent length in Missouri, ten farmers will have to pay for the erection of a couple of miles of wire, which can be put up for $30 a mile, and the instruments. Some person could be sent from the vicinity to the director of the service, and instructions given him in regard to the manner of operating the line and the management of the batteries. The cost of the line, therefore, to each farmer, would be, say, $75, which might be distributed over ten years. Mr. Nipher stated that in several localities the farmers will undertake it just as soon as the information can be furnished them. At the stations the lines could easily be made to terminate in the store of some merchant, who is anxious

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to secure the trade of the people on the line. This can be done at once in Missouri. The only thing necessary is for the state to appropriate a small amount of money to supply the persons with instruments for observations, rain-gauges, etc. The two things necessary to make it successful are information as to rainfall, and time of beginning and ending of rains.

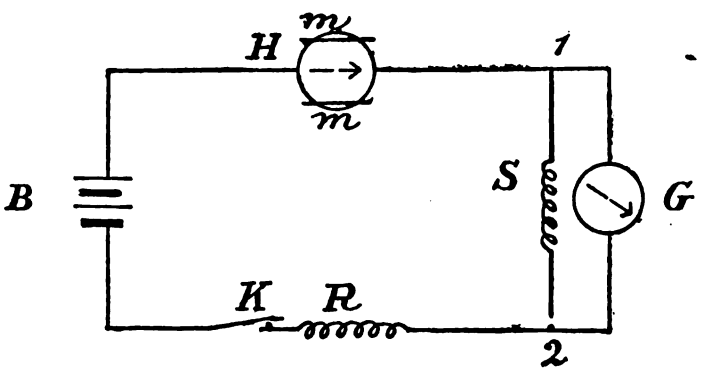
THE EXTENSION OF THE THEOREM OF THE VISIAL TO ROTARY OSCILLATION. By H. T. EDDY of Cincinnati, Ohio.

A METHOD FOR THE CALIBRATION OF A GALVANOMETER. By B. F. THOMAS of Columbia, Mo.

[ABSTRACT.]

A BATTERY of any sort is joined in circuit with a sensitive galvanoscope H, a galvanometer G, and any variable resistance R. When the circuit is closed at K, the current is so adjusted by varying R, as to give the highest desirable deflection of the galvanometer needle. The needle of H will be forced against the stops. By means of magnets m and m, the needle of H is brought

FIG. 1.



back to zero. If these magnets and the galvanoscope be undisturbed, the original current strength will be indicated when the needle stands at zero, whatever changes may have been made in the circuit. If now the shunt S be connected at 1, 2, and the re-

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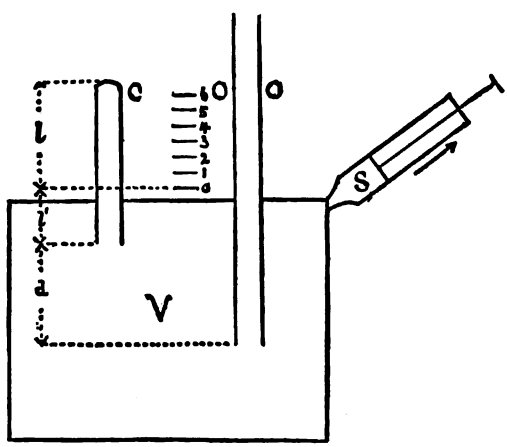
sistance of the shunt is made equal to that of the galvanometer (previously determined), and the needle of H brought back to zero by increasing R, a new deflection of the galvanometer needle will be produced, the deflection being that due to a current of one-half the strength of the original current. By giving to S values equal to 8, 3, 2, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., times the resistance of G, and bringing the needle of H to zero each time, deflections of G will result, due to currents whose strength are as 1, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, etc. The curve is then plotted with deflections and current strengths as co-ordinates. Any desired number of points in the curve may be obtained by giving S the proper values. The calibration may be checked by making a new adjustment for the unit current, so that the deflection of G shall be about two-thirds the first deflection, and proceeding as above. Plotting the new values obtained, the curves will coincide if the work is correct. If it be found desirable, the battery may be exchanged for another during the determination.

TWO FORMS OF APPARATUS FOR BOYLE'S LAW. By B. F. THOMAS of Columbia, Mo.

[ABSTRACT.]

THESE pieces are intended to enable one to adjust with accuracy and ease the mass of air to be experimented upon.

FIG. 1.



V is an iron cistern into which the open or pressure tube O, the

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closed tube C, and the reversible air-syringe S are screwed air-tight, and the cistern nearly filled with mercury. The syringe being connected for exhausting, and operated, air is withdrawn from C, until the mercury sinks to the bottom of the open tube, when air escapes from it, and rises through the mercury. No more air can be withdrawn from C. The mass of air remaining in C will evidently depend on the difference in depth of immersion of C and O. Let d = this difference, and let it be required to find such a value of d as will permit just enough air to remain in C to fill it from the zero of the scale, when at atmospheric pressure H. Let L = length of C from top to zero, and let l' = the length from zero to the open end of C. If now the mass of air which will fill the length l at H be expanded to fill the length l', the pressure H' at the bottom of C by Boyle's law is $H' = \frac{H\,l}{l+l'}$.

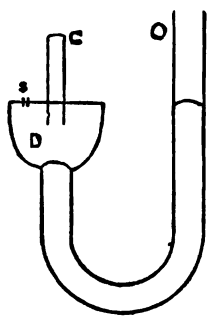
The pressure at the open end of $O = H$. The difference in pressure at the ends of C and O is that due to a column (d) of mercury. Hence $H' = H - d$.

Equating, $H - d = \frac{H\,l}{l+l'}$. $\therefore d = \frac{H\,l'}{l+l'}$.

On reversing the syringe, and forcing air in, the mercury will be found to rise and stand at zero in both tubes together. The demonstration is continued by forcing in more air.

A second form consists of two glass tubes connected by a strong rubber tube, and mounted on a stand with scales. The closed tube C is sealed into the screw-cover of an iron cistern D. Mercury being poured in, it will expel the air in D, and rise in an open screw-hole S in the cover. The hole being sealed by insertion of the screw, and O lowered, the air in C expands, filling C and D. On raising O, the mercury rises, and cuts off communication between C and D, preventing the return of some of the air. By making D of proper volume, the desired mass of air will remain in C. Let the volume of C above the zero = V. Let the entire volume of C = V', and the volume of D above the open end of C = V''. Following the above steps it will be seen that a volume V' at H becomes a volume $V' + V''$ at H'; also that a volume

FIG. 2.



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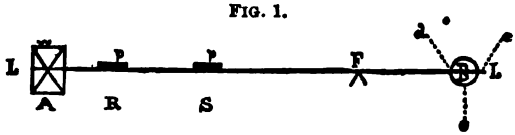
V at H becomes a volume V' at H'. Hence the proportion $V : V' :: V' : V' + V'', \therefore V'' = (V' - V)\frac{V'}{V}$. The use of the rubber tube is not new. This form is convenient because it answers for demonstrating the law at pressures above and below one atmosphere. Of course, for the latter, the air in D must be removed through S as at first.

A METHOD OF DETERMINING THE CENTRE OF GRAVITY OF A MASS.
By B. F. THOMAS of Columbia, Mo.

[ABSTRACT.]

A BAR, $L\ L$, is balanced on a knife-edge, F, so as to form a very sensitive balance. The body, B, of mass, M, is placed with a marked point in contact with a fine point, d; and another body

FIG. 1.



of mass, W, is placed at A, so as to nearly balance B. A small body of mass, p, is placed at S, to complete equilibrium. B is then rotated 180° horizontally, bringing its marked spot in contact with a second fixed point, e. Equilibrium is restored by placing p at R. The equations of moments in the two positions are, respectively,—

$$W \times AF + p \times SF = M\,(Fd + dc);$$

(c being the centre of gravity); and

$$W \times AF + p \times RF = M\,(Fe - dc);\ (ec = dc).$$

Subtracting the first equation from the second,—

$$p \times RS = M\,(de - 2\,cd);$$

$$\therefore cd = \frac{de}{2} - \frac{p \times RS}{2\,M};$$

cd is therefore the distance from the marked spot to a vertical plane containing the centre of gravity. Taking a second marked spot in the plane thus found, the operation is repeated, with the plane horizontal. This gives a second plane through the centre of gravity. A third operation, with the intersection of the two planes in the line de, locates the centre of gravity.

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A NEW HELIOSTAT. By B. F. THOMAS of Columbia, Mo.

THE KINETIC THEORY OF THE SPECIFIC HEAT OF SOLIDS.[1] By H. T. EDDY of Cincinnati, Ohio.

[ABSTRACT.[1]]

THIS paper applies to molecules of solid bodies the same principles which the author has elsewhere applied to gaseous molecules as to the distribution of vibratory energy among the different degrees of freedom possible to the molecules of a solid; and in particular it attempts to show that if the chemical atoms of all substances are formed of equal and similar ultimate atoms, then systems of solids which are in thermal equilibrium by radiation will also be in thermal equilibrium when brought into contact, *i. e.*, when the equilibrium depends upon collisions of the molecules.

THE KINETIC THEORY OF MELTING AND BOILING[2]. By H. T. EDDY of Cincinnati, Ohio.

[ABSTRACT.]

THIS paper discusses at length the two kinds of vibratory motion which the molecule of a solid body may have, rotary and translatory. It demonstrates that the mean kinetic energy of such an oscillatory motion as is possible for a molecule of a solid reaches a maximum value which can only decrease, whether the amplitude of the oscillation be increased or diminished, and that the only way in which it is possible to increase the mean kinetic energy of this kind of motion is to impart sufficient additional energy to change the motion into one of complete rotation. By such a process, greater freedom of motion is given to the molecules, and a large amount of energy becomes potential. This is regarded as explaining the phenomenon of liquefaction.

It is next shown by an extended mathematical discussion of the cohesive forces and resistance to compression by which molecules

[1] This paper was published in full in Science, Sept. 28, 1883.

[2] The principal conclusions in this paper were published in the Franklin Inst. Journal for July, 1883.

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hold each other at mean fixed distances, that the mean kinetic energy of the vibration of molecules about their mean positions also has a maximum value which can only be increased by removing them to such mutual distances that the cohesive forces no longer act. In this process a large amount of energy also becomes potential. This is regarded as the rationale of the phenomenon of vaporization.

It is further shown that on this theory it might very readily occur that the specific heat of a liquid should at first decrease and then increase, as Rowland has proved is the case with water, but that the specific heat could not at last decrease.

The cause of the relatively large specific heat of most liquids is treated. It is shown that the distribution of rotary velocities in free rotation such as the molecules of a liquid are supposed on this theory to have is such that the atoms of some small per cent of the molecules in any given mass must be torn asunder. What per cent of the liquid may be thus dissociated will depend upon the temperature and constitution of the liquid, it being smaller for the simpler liquids and increasing with the temperature. Electrolysis is an evidence of this action. Such dissociation sufficiently accounts for the generally high specific heats of liquids.

There is a general qualitative accordance of the theory with observed specific heats. A further confirmation of the theory is found in the clear explanation it affords of the existence of a critical temperature above which a vapor is incondensable by pressure alone; for when the mean kinetic energy of all the molecules of a liquid acquires a value greater than the maximum possible in a liquid state, the liquid is not only vaporized but necessarily becomes an incondensable gas and remains so.

Remarks on the Tracings of Self-registering Instruments, and the Value of the Signal-service Indications for June and July, 1883.[1] By Gustavus Hinrichs of Iowa City, Mo.

[1] A short notice of this paper is given in SCIENCE, Aug. 31, 1883, p. 285.

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The Utilization of the Sun's Rays for Warming and Ventilating Apartments. By E. S. Morse of Salem, Mass.

[ABSTRACT.]

The device consists mainly of a slaty surface, painted black, standing vertically upon a wall, outside the building, with flues to conduct warmed air to the inside. The slates are inserted in a groove, much as one might place glass in a frame. One made within the last year was three feet wide and eight long. It was placed where it received the sun's rays as directly as practicable. Its service was to warm a room used for a library. During an entire winter the room was thus made comfortable, except on a few of the coldest days. The current of air passing through it, when the sun's rays impinged directly upon it, was raised about 30°; it discharged 3,206 feet of warmed air in an hour. This was in the morning. At 11.45 the air of the apartment was raised 29°, with 3,326 cubic feet of air discharged; at 12.45, 29° and 4,119 feet; at 1.55, 24° and 3,062 feet; at 2.45, 20° and 1.299 feet. The room measured 20×14 and was ten feet high.

The apparatus works to most advantage in a room that is ventilated by an open chimney. But some very good results have been obtained in closed rooms. One was cited where the air, as it entered the room from the heater, was raised to nearly 40° above the outside temperature. In general, a difference of 30° to 35° can thus be secured during four or five working hours of the day.

Natural Snow Balls, or Snow Rollers. By Samuel Hart of Hartford, Conn.

[ABSTRACT.]

A more extended example of "Natural Snowballs" or "Snow Rollers" than has yet been recorded occurred in Connecticut and southern Massachusetts in February last. Snow had fallen on the 18th, the level surface of which had been covered with a crust from the freezing of a drizzling rain. On the evening of the 20th, a light but damp snow fell quietly upon this smooth crust, and afterwards a

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light wind sprang up from the south. In the morning the college campus, the city park, and open lots in every direction were strewn with masses of snow. Some were spherical, varying in diameter from one to nine or more inches; but most had the appearance of large old-fashioned muffs, of the shape of cylinders with a conical depression at each end reaching nearly or quite to the middle. The largest of these latter which I measured was eighteen inches long and twelve inches in diameter; and some were reported much larger. The path which each left behind it showed how it had been formed, a pellet of snow having been started by the wind and having gained not only in diameter but also in length as it rolled; so that a long isosceles triangle of snow was rolled up from its vertex, producing the peculiar shape just described. In many cases the path could be traced for twenty-five or thirty feet, and in some cases, it was reported, for sixty feet. The path of the round balls was, of course, of nearly the same width throughout. The direction of most of the paths was straight; but in some cases the snow had been carried by the wind in different directions, while occasionally it had been blown up a decided incline. The snow was not at all closely packed; it yielded to the least touch, and it was impossible to move one of the masses without breaking it.

The rollers were seen on the Connecticut river and along its banks for some forty miles below Hartford, as far north as Massachusetts, for some distance on the east, and as far west as the Housatonic valley. It seems quite within limits to say that they were spread thickly over all the open and level ground within more than 2500 square miles, and that they might have been numbered by millions.

TORNADO AT RACINE, WISCONSIN[1]. By P. R. HOY of Racine, Wis.

[ABSTRACT.]

FRIDAY, May 18, 1883, was a pleasant day; a slight shower of rain occurred soon after noon. At 6.45 there was almost continuous flashes of lightning, in the west, passing from a dark cloud coming from the northwest to a lighter colored, and less elevated

[1] The paper was accompanied by maps and photographs, illustrating facts relating to the tornado. For the discussion following the reading of this paper, see SCIENCE for Aug. 31, p. 281.

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cloud moving rapidly from the southwest, accompanied with thunder; these two clouds appeared to meet, producing a whirling, boiling tumultuous action, accompanied by a *roar* not unlike the passing of a heavy train of cars over a long bridge. The tornado passed the track of the C. and N. W. railroad, at just 7.10. The direction of the tornado was north of east; in six miles the storm advanced north 1¾ miles. Rain and hail fell over an area, commencing 1½ miles north of the tornado, and extending two miles north. Some of the hailstones were unusually large. Caledonia Station, on the St. Paul R. R., two miles north of the tornado track, was struck by lightning at 6.45 P. M. at the time of the hail-storm. The temperature of the atmosphere, just before the tornado struck Racine, was 64°. Fifteen minutes after it had passed the thermometer indicated 74°; this rise of 10° in so short a time was remarkable at that late hour. The barometer marked 29 inches at an elevation of 645 feet above the sea-level. There was only a pleasant breeze, rendering it comfortable to be out-of-doors at the time the tornado was passing 1¼ miles north of us. No rain fell in Racine, at the time, or for three days after the storm. My attention was called to a peculiar odor that pervaded the atmosphere soon after the tornado. This odor, which I then detected, was between sulphur and phosphorus; undoubtedly, it was ozone, resulting from electrical disturbance that filled the atmosphere at this time. It is proved that the onward motion was not more than thirty miles an hour, many families gained their cellars, and safety, after they had witnessed the destruction of houses situated not over twenty-five rods distant.

The gyratory motion, from right to left, is the principal, if not the only, factor in accounting for the destruction that followed in the immediate track of the storm.

The width of the tornado path was from ten to twenty rods, occasionally, however, much narrower; it was not straight but oscillatory, the variations being from 300 to 600 feet on either side of a straight line. Everything was thrown towards the centre of the track; not unfrequently objects on the outer borders were thrown at an acute angle west of either south or north, against the direction of the advancing storm. Still further, just beyond the line of total destruction, houses were pulled partly off their foundations; in these instances, the windows were thrown out on the storm side towards the path of the tornado.

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There were ten persons killed, all of whom save two lived near where the storm struck the city; they were surprised, not being aware of its approach. All were saved who gained their cellars, with one exception, that of a family living in a *brick veneered house*, the brick falling into the exposed cellar, killing two persons.

When the tornado passed over the lake, the sight was grand indeed, one long to be remembered; one, probably never before seen, for this is the only instance known of a tornado reaching Lake Michigan.

The water was whirled up at a great height, from 150 to 300 feet; the cloud would occasionally rise when the great volume of water would tumble back only to be caught up in a moment with redoubled force. While the water was being forced up in the centre by the whirling vortex, it was thrown over on the outside, thus forming irregularly circular cascades, swashing, splashing and foaming, in the wildest confusion. There were five or six accessary "water spouts," some of which, apparently, revolved around the "primary whirl;" one of these passed on the lake, at least one half mile south of the main storm. These accessaries were by no means insignificant either in size or force; only by comparison were they small. I saw two flashes of lightning in the tornado-cloud, while it was passing over the lake, but heard no thunder.

The storm left the lake covered with debris, for the distance of one mile from the shore. There was no well-formed funnel-shaped cloud; it was multiform, and resembled, somewhat, the smoke from a large factory chimney, when the condition of the atmosphere is such that the smoke flows over with whirling motion towards the ground. Imagine one hundred such columns, closely packed together, minus the chimneys, with volumes of light-colored smoke rising rapidly up in the centre with a gyratory motion, and you have a feeble representation of the Racine tornado cloud.

THE TORNADO OF MAY 18, 1883. By JNO. H. FRICK of Warrenton, Mo.

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SECTION C.

CHEMISTRY

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PAPERS READ.

Report of the Committee on Indexing the Literature of Chemical Elements.

The undersigned, a committee appointed at the Montreal meeting of the American Association for the Advancement of Science, "to devise and inaugurate a plan for the proper indexing of the literature of the chemical elements," respectfully submit the following report.

The members have conferred with each other orally and by correspondence. Several plans have been suggested, and their merits discussed. Three methods of collecting material for the indexes may be named:—

1. Reviewing the Catalogue of Scientific Papers published by the Royal Society (8 vols. 4to).

2. Indexing special journals by different individuals, and collating the matter.

3. The independent plan, whereby each chemist indexes all the journals available to him with reference to a given element, in which he is presumably especially interested.

Each of these schemes is open to objections, and has its difficulties. The first would necessitate an enormous amount of clerical labor, for which volunteers would scarcely be secured; besides, data previous to 1800 could not be obtained from this catalogue.

The second involves, also, securing a large number of self-sacrificing volunteers; and both plans would require a vast amount of editorial work on the part of this committee.

The third plan seems, to a majority of the committee, the only feasible one at present. On the independent plan, seven indexes have already been compiled.

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The best arrangement of material has also been considered; and here again a threefold problem occurs:—

1. Chronologically.
2. Alphabetically, by authors.
3. Topically.

The committee do not venture to dictate to independent workers, but recommend the chronological arrangement, with the understanding that a topical index accompany each monograph.

The best channel of publication has also been considered by the committee. All the indexes hitherto published have been printed in the Annals of the New York Academy of Sciences; and the Academy has generously offered, through its officers, to continue its good work. The Smithsonian Institution further agrees to distribute, free of expense, all circulars and documents in furtherance of this undertaking; an offer which is of greatest importance, and for which this committee expresses sincere thanks.

Since the appointment of the committee, Mr. Webb's Index to the Literature of Electrolysis has been published in the Annals of the New York Academy of Sciences; and several chemists have expressed a willingness to coöperate in the proposed undertaking. Prof. R. B. Warder of Cincinnati has promised an index to the literature of the velocity of chemical reactions; Dr. Henry Leffmann of Philadelphia proposes to index the important element arsenic, and Professor C. E. Munroe offers a Bibliography of Explosives.

H. C. BOLTON, *Chairman;* IRA REMSEN; F. W. CLARKE; A. R. LEEDS; A. A. JULIEN.

ON THE SUB-AQUEOUS DISSOCIATION OF CERTAIN SALTS. By JOHN W. LANGLEY and CHAS. K. MCGEE of Ann Arbor, Mich.

IN his "Leçons sur la Dissociation" (Paris Chemical Society, March and April, 1864), Sainte-Claire Deville directs the attention of chemists to certain properties which salts in solution display: such as contraction of volume, the evolution or absorption of heat, variations in density, etc. But he particularly calls attention to some of the chemical changes which the salt appears to

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undergo by the mere fact of solution, changes which increase in amount with the quantity of water added and which cause the salt to act finally, rather like a mere mixture of its components, than like a compound such as it is known to be in its solid state.

Deville considers that there is no absolute distinction between solution and chemical union; the difference is one of degree rather than one of kind, for out of the possible unions and rearrangements between atoms and molecules which are embraced in the term "chemical changes," some are synthetic, some analytic, and some partake of both processes. Now in Deville's mind solution is of the last order; synthetic and analytic changes always occur in solution, only the latter is the predominant one, so that there is absorption of heat accompanying contraction of volume instead of evolution of heat as would be expected *á priori;* and in addition a constant tendency for the salt to be changed to simpler components.

Thus, when a salt is dissolved in water there is a combination of the solid with the whole of the water, but at the same time the salt undergoes a species of vaporization, for its molecules are distributed through the entire mass of the water, and consequently occupy a larger amount of space, just as a vapor would do if produced by the action of heat. The heat units necessary to perform this vaporization are taken from surrounding bodies, and are, therefore, virtually unlimited in amount; now as the heat absorbed increases with the degree of dilution, the quantity will finally become sufficient to completely dissociate and resolve, even into its elements, any salt when dissolved in a sufficiently large body of a neutral solvent such as water.

We have here briefly recalled Deville's theory of dissociation as the necessary result of solution, because we have made some experiments in one limited direction which were suggested to us by the theory in question.

Every chemist is familiar with the classic researches of Graham on diffusion, and of the admirable work of Berthelot on thermo-chemistry, and of the bearing on Deville's theory of solution. We have applied the theory to account for the changes in the degree of acidity which some salts display when dissolved.

It is a well-known fact that many salts which are neutral by theory are not neutral to litmus paper; for instance, cupric sulphate in solution is acid to litmus, and it will remain acid however much

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potassic hydrate may be added to it, so long as all the copper is not precipitated. It is also a well-known elementary fact that certain salts whose metals form very feeble bases are decomposed with the formation of basic precipitates, while the greater part of the acid is found in the liquid, for example, bismuth and antimony chlorides.

If it be true that salts tend to dissociate when dissolved, and are actually decomposed when sufficiently diluted, we should expect that they would break up along the lines of least resistance into simpler molecules first, and ultimately into free atoms. Now when water is present we may take three views of the condition of the salt. We may suppose it to be merely attached to the water by a species of physical adhesion, the salt nucleus remaining unchanged when the quantity of water is very small; or second, we may consider the water and the salt to be united into a new group which acts as a compound molecule, so long as the body of the solvent is so small that the amount of diffusion is negligeable. Then for sodic sulphate in two molecules of water we should have, for the first case [Na_2SO_4 ,$2H_2O$], and for the second [2(NaOH), H_2SO_4], where the comma indicates a molecular, as distinguished from an atomic, union.

Finally, we may decline to theorize about the initial state of the salt and water, and consider that we have a certain quantity of different kinds of matter held momentarily in equilibrium by internal and external forces, but which will form definite associations with each other whenever the external forces change. On this view the above symbols might be written [$Na_2H_4SO_6$], and we should have no theory as to whether the Na was combined with H, with S, or with SO_4.

Researches in thermo-chemistry tell us that the heat of combination between H_2SO_4 and 2NaOH is less than the heat of formation of sodium hydrate starting with metallic sodium and water, or of sulphuric acid starting with SO_3 and water. The line of least resistance in the group $Na_2H_4SO_6$ probably, therefore, passes through the comma in the symbol 2(NaOH), H_2SO_4 and indicates that the first stage of dissociation will be into free sodium hydrate and free sulphuric acid. The general theory of dissociation shows also that this change will be only partial for all finite ratios between the quantities of the salt and the water; it should first become apparent as traces, and proceed under increasing dilution

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to amounts of free acid which could be quantitatively estimated.

When antimony or bismuth chlorides are diluted, we find the metals separating from the liquid as insoluble basic chlorides, whose action on litmus paper is however very slight; that is, the basic element has very feeble alkaline characters. On the other hand, the power to redden litmus possessed by the mineral acids H_2SO_4, HCl and HNO_3 is very marked and is of nearly the same intensity in the three, while the corresponding power to blue the litmus possessed by the hydrates varies greatly. It is very strong in the alkali metals, but is quite feeble in metallic hydrates such as those of zinc, iron and copper. If then a salt actually separates partially into free acid and free base when dissolved, and if the power of the base to turn litmus blue is not the exact quantitative counterpart of the acid to turn it red, there must necessarily occur, with differing degrees of dilution of the solution, differing powers of coloring litmus possessed by any salt, even if it were perfectly neutral, both by composition and to litmus, when it was in the solid state.

To test this hypothesis we have used the following method:

Chemically pure salts were obtained in saturated solutions. There was also prepared a standard solution of pure sulphuric acid containing one gramme of acid to the litre of solution. And, finally, a standard solution of litmus was provided which was made as nearly neutral as possible.

Into a series of four inch test tubes were introduced $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, etc., c.c. of this standard acid and each of them then diluted with the litmus solution to ten c.c. The tubes consequently gave a series of red tints from neutral purple to decided red. Tube No. 1 contained $\frac{1}{10000}$ gramme of H_2SO_4; No. 2, $\frac{2}{10000}$; No. 3, $\frac{3}{10000}$, etc. These quantities were added to amounts of litmus solution which were nearly the same, being the quantity necessary to complete the volume to 10 c.c. (The litmus actually ranged from 9 c.c. to 9.9 c.c.)

Saturated solutions of each of the following salts were examined:

$ZnSO_4$, $CuSO_4$, $PbSO_4$, $AgSO_4$, $CaSO_4$, Na_2SO, $FeSO_4$, $HgSO_4$, $MnSO_4$, $Al_2(SO_4)_3$, $Fe_2(SO_4)_3$, $ZnCl_2$.

In each case 1 c.c. of the salt solution was introduced with a

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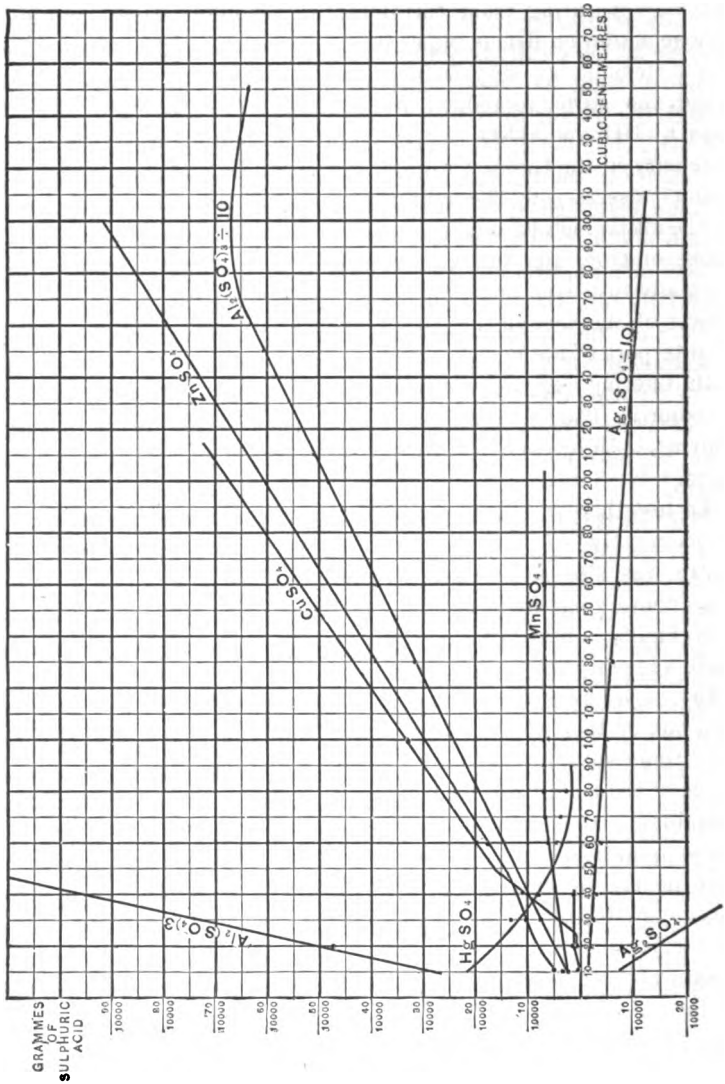
test tube and enough litmus solution added to make 10 c.c. The resulting color was compared with the scale of colors made with



the sulphuric acid by looking across the test tubes. 10 c.c. of litmus solution were now added and the resulting color again re-

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corded in terms of the color scale. Successive dilutions up to 100, and in some cases up to 400 c.c., were made and the colors noted.

Let 1 c.c. of some salt solution be diluted with litmus to 10 c.c., and let the color produced be the same as that of tube No. 7. Evidently $\frac{7}{1000}$ gramme of H_2SO_4 is equivalent to the salt in its reddening effect on litmus solution. If now we dilute the salt solution to 40 c.c. and find that it gives the same tint as tube No. 5, it is evident that $\frac{20}{10000}$ gramme H_2SO_4 would be required to produce the same reddening effect as that produced by the salt; for $\frac{5}{1000}$ gramme H_2SO_4 is needed to make 10 c.c. equally red, and consequently the 40 c.c. of the diluted salt represent four times the reddening effect of $\frac{5}{1000}$ gramme of acid in 10 c.c. of litmus. In this way the number of $\frac{1}{1000}$ gramme of H_2SO_4 required to produce the same reddening effect for various dilutions of each salt is found and represented as ordinates in the curve.

In the diagram, the numbers on the horizontal axis indicate the number of cubic centimetres to which one centimetre of salt solution has been diluted with the litmus solution. The numbers on the vertical axis indicate the number of $\frac{1}{1000}$ grammes H_2SO_4 which could be substituted in place of the salt in any given case and have the same reddening effect.

$CaSO_4$ and Na_2SO_4 acted like neutral salts and did not change the litmus solution, either with low or high degrees of dilution.

Ag_2SO_4 was the only salt found to change the solution *blue* instead of red.

$Fe_2(SO_4)_3$ and $FeSO_4$ gave no reliable results on account of a dirty precipitate which prevented all color tests. Both, however, made the litmus distinctly red, and $Fe_2(SO_4)_3$ seemed to be as acid as $Al_2(SO_4)_3$.

$HgSO_4$ was made by drying the crystals of $HgSO_4$ in free H_2SO_4 upon filter paper and dissolving as much as possible in water. After the basic mercuric sulphate had settled out, the clear solution above was used in the test. Probably the curve is simply that of free H_2SO_4.

When largely diluted $Al_2(SO_4)_3$ gave a precipitate which interfered somewhat with the tests. The same is true of $CuSO_4$. $ZnCl_2$ could not be examined quantitatively for the same reason, although it reddened the litmus.

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Much difficulty was experienced at first from the fact that the various color tubes gradually changed back to a neutral tint after standing a day or two. If a tube, to which acid had just been added and mixed by stirring, were strongly shaken for one or two minutes, it would be found to have changed sensibly towards a blue. Owing to this instability of color the standard tints were prepared afresh every day for each day's work, and the amount of stirring was made as nearly as possible alike for all experiments.

This apparent loss of acid, shown by a real loss of red coloration, is probably due to oxidation which seems to regenerate the blue color. By working with freshly boiled solutions and making the mixtures under a film of paraffine oil, the change of color was rendered much less, and, therefore, we have little doubt that the complete exclusion of dissolved oxygen would have entirely prevented the change of tint.

It is perhaps hardly legitimate to draw general conclusions from a limited number of experiments, especially of work like this, where there are several unknown factors, such as the possible differential attraction of the coloring matter for the several bases, etc. Still, taking the results as they stand, this provisional conclusion may be drawn.

First. Sulphates of the alkali metals, except silver, seem to be strictly neutral under all degrees of dilution.

Second. Sulphates of the RSO_4 type, where R is a dyad metal, show an amount of dissociation which is in proportion to the degree of dilution, and hence their curves are represented by a nearly straight line.

Third. Aluminic sulphate and other double triads are not neutral when concentrated. When diluted they rapidly become strongly acid, and when the dilution exceeds a certain limit they seem to lose acid at a decreasing rate. Hence their curves are not uniformly straight lines.

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Suggestions for computing the Speed of Chemical Reactions.

By Robt. B. Warder of North Bend, Ohio.

[abstract.]

This paper urges a thorough discussion of data upon the subject indicated in its title, for the following reasons: 1. To discover and investigate the causes of certain discrepancies between published observations and current theories. 2. To obtain more definite information as to the nature of certain reactions and the conditions determining their speed. 3. To afford numerical data for a fuller study of relations between the speed of reactions and other physical constants. 4. To suggest fruitful lines for further research in chemical dynamics.

As instances of the need of such discussion, in the determinations by Professor Menschutkin, of the speed and limits of the etherification of the several alcohols and acids, numbers are given for the "initial speed" of reaction, which express the average speed during the first hour, and are not proportional to speeds during the first minute. Prof. L. Meyer in his *Dynamik der Atomen* passes very lightly over both the theory and the observations of speed during a reaction.

The prevalent theory of the action of mass is expressed by the equation, $\frac{du}{dt} = kuv$, in which the differential expresses the rate of change in any substance, u and v represent the masses taking part in the change, and k is a constant. Observations by Ostwald and others indicate that some modifications of this theory are needed. Determinations of the speed of reaction require special care, both to measure time in relation to mass, and to control temperature and other conditions. The chemical section of the Ohio Mechanics' Institute has recently undertaken some work of the sort, and invites coöperation.[1]

The following provisional system of units is suggested: for volume, one cc.; for mass, the chemical equivalent expressed in mg.; and for time, one hour. The unit of speed would be the transformation of unit of each active body per unit of volume and time. Possibly the comparison of the constants of speed or of chemical affinity with those of heat, electricity, etc., could be better made

[1] Sci. Proc. O. M. I. 2, 51.

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from the unit of one second or 1,000 seconds. At least two observations of time and two of mass are required; and preferably several, to determine the limits of error. Determinations which do not accord with the hypothesis that the speed and the product of active masses vary in the same ratio, need special investigation. In reciprocal reactions, some of the ratios derived from observations on chemical equilibrium, may be combined with constants of speed already determined, to deduce other constants. By bringing all the facts into systematic order, these data can be made of use for comparison in other fields of physical science.

For the convenience of those who may be interested, a table is here given of some of the papers containing or discussing determinations of speed. References are given to (JB.) Jahresbericht der Chemie; (Ber.) Berichte deut. chem. Gesell.; (C. S. J.) Journal of the Chemical Society; (P. M.) Philosophical Magazine; (J.p. C.) Journal für prak. Chemie; (A. C. J.) American Chemical Journal; (Sci. Proc.) Scientific Proceedings of the Ohio Mechanics' Institute; and (I. A. R.) Indiana Agricultural Report. Some of the papers referred to have only an indirect bearing on the speed of reactions. They are grouped according to author and subject, and placed as nearly in historical order as consistent with this arrangement.

WITTWER	JB. 1855, 172, 174	Decomposition of water by chlorine.
BUNSEN (and ROSCOE)	" 1855, 173	$H + Cl = HCl$ under the influence of light. "Photochemical induction" is discussed.
	" 1856, 185	
	" 1857, 37, 43, 49	
	" 1859, 32	
A. BAEYER	" 1857, 48	Br and milk sugar.
DRAPER	" 1857, 51	Reduction of ferric oxalate.
BERTHELOT (and ST. GILLES)	" 1861, 591	Etherification, etc.
	" 1862, 386	
	" 1863, 458, 463	
	" 1877, 25, 88, 89	
	" 1878, 10, 14, 15, 513	
	" 1880, 83, 85	
VAN'T HOFF	Ber. **10**, 669	" "
MENSCHUTKIN	JB. 1877, 321	" "
	" 1878, 513, 538	
	" 1879, 313, 314	
	" 1880, 381, 600, 752, 816	
	" 1881, 15	
	Ber. **15**, 162, 248, 721, 1445, 1572	
	" **15**, 1615, 2237, 2502	Acetanilide. [Compare L. Meyer, Ber. **15**, 1977.]
	" **15**, 2512, 2738	Tertiary amyl acetate. [Compare Warder, Sci. Proc. **2**, 2.]

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HARCOURT (and ESSON)	JB.	1864, 9	Oxidation of $C_2 H_2 O_4$ by $K_2 Mn_2 O_8$ and of HI_3 by $H_2 O_2$.
	"	1866, 10	
	"	1867, 23	
Miss REED	"	1875, 14	
GULDBERG and WAAGE	"	1866, 15	General theory, with numerous illustrations and deductions.
	"	1879, 22	
GLADSTONE (and TRIBE)	"	1871, 13, 15, 16	Replacement of metal in salts.
	"	1875, 14	Metathesis of salts.
LEMOINE	"	1871, 117	
	"	1875, 92	$H + I = HI$ and the reverse.
BUCHANAN	"	1871, 115, also	Halogens, and carbon compounds. [Compare Urech.]
	Ber.	**4**, 863	
JACKSON	JB.	1879, 340	
	"	1880, 482	
	A.C.J.	**3**, 252	
G. C. THOMPSON	JB.	1879, 594	
HELL (and URECH)	"	1880, 753	
	Ber.	**15**, 273, 987	
	"	**16**, 1144	
URECH	JB.	1880, 386, 697	Br etc. Isobutyl aldehyde.
	"	1880, 1021	Inversion of cane sugar. [Compare Warder.]
	Ber.	**15**, 2130, 2457	
	"	**16**, 762	
	"	**14**, 340	
	"	**15**, 2685	Cu S O_4 and glucose.
LEEDS	JB.	1876, 165, 166	Reduction of Ag NO_3.
LUNGE	"	1876, 13	Action retarded by indifferent bodies. [Compare Hood.]
DUNN	"	1877, 30	
BOGUSKI and KAJANDER	"	1876, 12	Solution of Ca CO_3 etc. in acids.
KAJANDER	"	1880, 11	
	"	1881, 24, 211	
PAWLEWSKI	"	1880, 11	
OSTWALD	"	1875, 11	Bi Cl_3 + H_2 O. [Compare Muir.]
	"	1876, 23	Volume-chemical studies
	"	1877, 28, 1033	
	"	1878, 27	
	"	1879, 24, 25	
	"	1880, 8	
	"	1881, 11, 12	
	J.p.C.	[2] **25**, 1, 452	
	"	[2] **26**, 384	Decomposition of acetamide.
	"	[2] **27**, 1	Abstract in Sci. Proc. **2**, 44.
MUIR (and others)	JB.	1877, 19, 27	
	"	1879, 21	Bi Cl_3 + H_2 O. [Compare Ostwald.]
	"	1880, 10	
	Ber.	**14**, 358, 360	
	"	**15**, 394, 395, 921, 958	
	C.S.J.	**39**, 21	
WRIGHT (and others)	JB.	1877, 91	
	"	1878, 232, 234	
	"	1879, 27	
	"	1880, 7, 85, 151	Chemical affinity and electromotive force.
	"	1881, 91	

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HOOD	JB. 1878, 16	
	" 1879, 21	
	P.M. [5] **13**, 419	Retarding influence of salts. [Compare Lunge and Dunn.]
MILLS (and others)	P.R.S. **28**, 268, 270, 273, or C.S.J. **38**, 437, 438, 458	
POTILITZIN	JB. 1879, 29	
	" 1881, 12, 15	
	Ber. **15**, 918, 3099	Metathesis. [Compare Beketow.]
BEKETOW	JB. 1881, 14	
	Ber. **16**, 775	
VILLIERS	JB. 1880, 381, 595	Etherification of mineral acids.
BEAMER and CLARKE	" 1880, 906, or A.C.J. **2**, 329	Decomposition of organic salts by heat.
WARDER	Ber. **14**, 1361	$C_2H_5 \cdot O \cdot C_2H_5O + NaOH$.
	A.C.J. **3**, 55, 340	
	Proc. A.A.A.S. **30**, 70	
	Sci. Proc. **1**, 105, 166, 167	
	" **2**, 2,	
	Science **2**, 176	Dissociation of brass.
RIBAN	JB. 1881, 665, 666	Decomp. of formates and acetates.
HUSTON	I.A.R. 1882, 230	Citrate on reverted phosphate. See Sci. Pro. **2**, 134.
CALM	Ber. **15**, 609	} Naphtylamine and diuaphtylamine.
BENZ	" **16**, 8	
SPINDLER	" **16**, 1252	Nitrification of benzol.

AMERICAN BUTTERS AND THEIR ADULTERATIONS. By H. W. WILEY of Washington, D. C.

[ABSTRACT.]

A SERIES of elaborate experiments and analyses of various samples of butter, oleomargarine, tallow, and lard, has been made by the author. The paper, which will be printed in full in the report of the Commissioner of the United States Department of Agriculture, contained a description of the methods employed by the author. He takes a weighed quantity of the butter, puts it in a sand-bath, and dries for two hours at 100°. The curd or caseine is determined by ignition: five grammes are used for the purpose. Dry combustion in a tube is difficult and unsatisfactory: he therefore uses the moist-combustion method, with permanganate and nesslerizing. The amount of salt he considers important. It is usually determined by ignition, and

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weighing the residue; but he found that so much chlorine was thereby lost, that the result was not trustworthy. He washes the butter by shaking it in a separating funnel with hot water, and then determines the chlorine with standard silver nitrate and potassium bichromate as an indicator.

Several novelties were devised for these analyses. One is for ascertaining the melting-point. The butter is packed in a U-shaped tube, of which one leg is longer than the other. The tube is placed upright in a vessel containing sufficient mercury to overflow the top of the tube. This vessel is placed in another containing water, and heat is applied beneath. The water, heated, in turn heats the mercury surrounding the tube, until the contents of the tube are melted. As soon as the melting takes place, the melted material leaves the tube, and floats on the surface of the mercury. Another method consisted in laying platinum wires upon the sample of butter, etc., heating the wires, and noting the heat required to cause them to disappear by sinking into the sample. These methods determined not only the melting-point of samples of butter, oleomargarine, tallow, and lard, but also of the fatty acids. But the variations in the melting point of genuine butter are so wide, that no certain conclusion can be arrived at by comparison with melting-point of oleomargarine, etc., to test the question of genuineness. Thus it was found that first-rate butter from an Alderney cow at one time, owing to special feeding, had a higher melting-point than oleomargarine; while a few weeks later, with different food, the same cow supplied milk from which was made butter with a lower melting-point than oleomargarine.

In regard to other tests, concerning which full details were given in the paper, it may be briefly stated, that, as a general rule, the amount of caseine present in pure butter is much greater than in oleomargarine. The specific gravity of genuine butter is higher. The saturation coëfficient for the insoluble acids in the genuine butter is low, in the imitations it is high. The author placed more reliance on tests for saturation coëfficient than on other methods. The soluble fatty acids in pure butter range from three to five per cent; while in oleomargarine, tallow, etc., they are either absent, or show a mere trace. The author also called attention to polarization tests. The genuine butter gives a uniform field in polarized light: oleomargarine gives a field with mottled and crystalline structure. He had made no analysis of

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butter known or suspected to be adulterated by mixture. He considered it unwise to decide the question of genuineness from any one of the constituents or conditions of a sample; believing that all the different tests should be brought to bear. He presented elaborate tables of analyses of different kinds of butter, etc.; specifying for each the place of purchase, name sold by, price, color, percentage of water, of caseine, of salt, specific gravity at 40°, melting and solidifying points, percentage of soluble and of insoluble acids, and the melting and solidifying points and the saturation equivalent of the insoluble acids.

ON γ-DICHLORDIBROMPROPIONIC AND γ-DICHLORBROMACRYLIC ACIDS. By C. F. MABERY of Cleveland, Ohio, and H. H. NICHOLSON of Lincoln, Neb.

[ABSTRACT.]

WHEN dry chlorine is passed through β- dibromacrylic acid, the reaction is easily accomplished, and the product may be purified without difficulty by crystallization from carbonic disulphide. This acid is very sparingly soluble in water, more soluble in hot than in cold carbonic disulphide and chloroform. It melts at 100°. Its salts were carefully studied, but the silver salt was found so unstable that it could not be prepared in a state of purity. Since β-dibromacrylic acid has, without doubt, the form

$$\begin{array}{l} CBr_2 \\ \| \\ CH \\ | \\ COOH \end{array}$$

, the chlorine addition product would have

$$\begin{array}{l} CBr_2Cl \\ | \\ CHCl \\ | \\ COOH \end{array}$$

the form . This acid is entirely decomposed when heated with an excess of any alkaline hydrate. If, however, the reaction is allowed to progress in the cold, keeping the hydrate

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in slight excess, the elements of hydrobromic acid are easily removed, with the formation of the corresponding dichlorbromacrylic acid. In order to distinguish this from two other products which have already been obtained, it will be called the γ-acid. It is prepared by the action of baric hydrate upon γ-dichlordibrompropionic acid, and the reaction proceeds so rapidly that it is difficult to keep the solution alkaline. Upon acidifying the baric hydrate solution with hydrochloric acid, γ-dichlorbromacrylic acid is precipitated partly as a crystalline solid, and is easily purified by crystallization from hot water. It is sparingly soluble in cold, readily in hot water, and readily soluble in alcohol, ether, carbonic disulphide, and chloroform. It crystallizes in pearly white scales, which melt at 78° to 80°. For further identification, the acid was analyzed, and its salts submitted to careful study.

ON THE FORMATION AND CONSTITUTION OF CHLORDIBROMACRYLIC ACID. By C. F. MABERY of Cleveland, Ohio, and RACHEL LLOYD of Louisville, Ky.

[ABSTRACT.]

IN a paper published in the Proceedings of the American Academy of Arts and Sciences, vol. xviii, p. 41, F. C. Robinson and one of us (C. F. M.) mentioned certain experiments in which we had tried to obtain an addition-product of brompropiolic acid with chlorinemonobromide. Although the results then obtained were not of an encouraging nature, it nevertheless seemed possible to obtain the desired product. On again taking up the subject the importance of obtaining chlorinemonobromide absolutely free from any excess of bromine was evident, inasmuch as a small quantity of free bromine would form tribromacrylic acid which could not be separated from the product by crystallization. After several trials it was found that, if chlorine was passed into bromine cooled to 0° until the latter was saturated, the product dissolved in chloroform and this solution saturated at 0°, no appreciable amount of tribromacrylic acid was formed when brompropiolic acid was added to it.

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The acid was added slowly, keeping the chlorinemonobromide in excess, and this solution allowed to stand for some time. Evaporation of the chloroform left a solid residue which was easily purified by crystallization from hot water.

This substance melts at 104°, and is sparingly soluble in cold, more so in hot, water and carbonic disulphide, and quite soluble in chloroform. By slow evaporation it separates from a solution in carbonic disulphide in triclinic prisms which have been submitted to a careful study by Mr. O. W. Huntington.

The composition of this acid was determined by analysis, and it was further characterized by a study of its salts and a determination of its solubility in cold water.

The following salts were examined:

Argentic chlordibromacrylate, $AgC_3ClBr_2O_2$.
Rhombic plates, permanent in diffuse daylight.
Baric chlordibromacrylate, $Ba(C_3ClBr_2O_2)_2 \cdot 3H_2O$.
Calcic chlordibromacrylate, $Ca(C_3ClBr_2O_2)_2 \cdot 2\frac{1}{2}H_2O$.
Potassic chlordibromacrylate $KC_3ClBr_2O_2$.

The potassium salt was so deliquescent that its crystal water could not be determined. As a mean of two determinations the solubility of the acid in water at 20° was found to be expressed by the percentage 5.74.

Although previous experiments have shown (Proc. Amer. Acad., vol. xvii, 209, and vol. xviii, 45) that chlortribrompropionic acid, like other substituted propionic acids containing several halogen atoms, is readily decomposed when heated with baric hydrate in aqueous solution, when the action takes place in the cold in a slightly alkaline solution, the elements of hydrobromic acid are easily removed with the formation of a chlordibromacrylic acid. We, therefore, took advantage of this reaction to compare the resulting product with the addition-product above mentioned of brompropiolic acid. The object of this comparison was to establish the constitution of the chlordibromacrylic acid and thus determine the manner in which the elements of chlorinemonobromide united with brompropiolic acid. Chlortribromproprionic acid was allowed to stand for two days with baric hydrate in aqueous solution, sufficiently in excess to give a distinct alkaline reaction. On acidifying with hydrochloric acid, an oil was precipitated which soon solidified and more of the acid was ob-

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tained by extracting the solution with ether. The product was purified by crystallization from hot water in which it is much more soluble than in cold. From carbonic disulphide it crystallizes in rhombic prisms which melt at 99°. Its identity was established by analyses, and several of its salts were made and analyzed.

The silver salt proved to be so unstable that it could not be prepared in a form sufficiently pure for analysis.

Baric chlordibromacrylate, $Ba(C_3ClBr_2O_2)_2 \cdot 3H_2O$.

Calcic chlordibromacrylate, $Ca(C_3ClBr_2O_2)_2 \cdot 4H_2O$.

Potassic chlordibromacrylate, $KC_3ClBr_2O_2$.

The latter salt was so deliquescent, its water of crystallization could not be determined.

Two determinations of the solubility of this acid in water at 20° gave as a mean the percentage 2.60.

A difference in structure of the acid obtained by the two methods seems to be fully established by the different melting points, the wide difference in stability of the silver salts, and the different degrees of solubility in cold water. If the structure

$$\begin{array}{l} CClBr_2 \\ | \\ CHB_2 \\ | \\ COOH \end{array}$$

assigned to chlortribromacrylic acid (Proc. Amer. Acad., vol. xviii, 46) is correct, eliminating the elements of hydrobromic acid must of necessity give an acid of the form

$$\begin{array}{l} CClBr \\ \| \\ CBr. \\ | \\ COOH \end{array}$$

Chlorinemonobromide can combine with brompropiolic acid in one of two ways:

$$\begin{array}{lcccc} & & & (I) & (II) \\ CBr & & & CClBr & CBr_2 \\ \| & & & | & | \\ C & + ClBr & = & CBr \quad \text{or} & CCl \\ | & & & | & | \\ COOH & & & COOH & COOH \\ \text{Brompropiolic acid.} & & & \text{Chlordibromacrylic acid} & \end{array}$$

The first form (1) is identical with that of the acid obtained from chlortribrompropionic.

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Since the two products have been shown to be essentially different in their properties, the second form (II) seems more probable for the addition-product obtained with chlorinemonobromide. If this view is correct, it follows that in this reaction the weaker halogen unites with the terminal carbon atom

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ON ORTHOIODTOLUOLSULFONIC ACID. By C. F. MABERY of Cleveland, Ohio, and G. M. PALMER of Cambridge, Mass.

[ABSTRACT.]

WHEN sulphuric anhydride in small excess is added gradually to orthoiodtoluol, chemical reaction immediately ensues as shown by a rapid development of heat.

This reaction is probably expressed by the equation:

$$C_6H_4I_0CH_3 + SO_3 = C_6H_3I_0CH_3SO_2OH.$$

The resulting product was converted into the barium salt by treating it with an excess of baric carbonate after diluting with water, the solution filtered and concentrated by evaporation. On cooling, the barium salt crystallized in clusters of flat prisms or needles which were rather sparingly soluble in hot water, much less so in cold. It was purified by successive crystallizations from hot water and the water of crystallization determined at 100°. A determination of barium in the anhydrous salt gave the formula $Ba(C_7H_6ISO_3)_2$, or for the hydrous salt $Ba(C_7H_6ISO_3)_2.1\frac{1}{2}H_2O$. On evaporating the mother liquors of the several crystallizations a salt was deposited which differed somewhat in form from the first, but it was found to contain the same percentage of crystal water.

The free acid was prepared from the barium salt by precipitation with sulphuric acid. When the water was removed by evaporation, and the residue allowed to stand over sulphuric acid, the sulphonic acid became a thick syrupy liquid which refused to crystallize.

From the free acid the following salts were prepared.

Plumbic orthoiodtoluolsulphonate, $Pb(C_7H_6ISO_3).2H_2O$.

Calcic orthoiodtoluolsulphonate, $Ca(C_7H_6ISO_3).2\frac{1}{2}H_2O$.

Argentic orthoiodtoluolsulphonate, $AgC_7H_6ISO_3$.

We did not succeed in identifying a second acid, either in the mother liquors of the barium salt or in the free acid itself.

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THE COMPOSITION OF AMERICAN WHEAT AND CORN. By CLIFFORD RICHARDSON of Washington, D. C.

[ABSTRACT.]

THIS paper gave an account of results obtained by the author in his work for the U. S. department of agriculture, and the paper will appear in full in the Report of the Commissioner of the Department. More than 200 analyses of wheat, and 100 of corn, have been made during the last two years under his supervision. It appears that while our wheats are of somewhat lighter weight, they contain less water, about the same ash, more oil, less fibre, and less albumen, than the foreign wheats. Among our wheats, only those from Colorado, Dakota, and Minnesota, equal the European in albuminoids and in size of grain. The wheats of the Atlantic states are poor in nitrogen. Corn, compared with wheat, contains twice as much oil, less starch, more water and fibre, and less of albuminoids. The following table gives a condensed statement of the analyses:—

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AVERAGE COMPOSITION OF AMERICAN CORN (MAIZE).

	Weight of 100 kernels.	Water.	Ash.	Oil.	Carbhydrates.	Fibre.	Albuminoids.	Nitrogen.	Weight of largest 100 kernels.	Weight of smallest 100 kernels.	Highest albuminoids.	Lowest albuminoids.	No. of analyses.	No. of weights of 100 kernels.	Analyst.
	Grams.	*Pr. ct.*	*Pr.ct.*	*Pr.ct.*	*Pr. ct.*	*Pr.ct.*	*Pr. ct.*	*Pr.ct.*	*Grams.*	*Grams.*	*Pr. ct.*	*Pr. ct.*			
America	36.910	10.04	1.52	5.20	70.69	2.09	10.46	1.67	52.679	23.605	13.65	7.00	114	61	
Northern States	37.320	9.98	1.54	5.11	71.32	1.41	10.64	1.70	44.147	30.610	13.65	7.53	30	8	
South	.....	8.96	1.37	4.94	72.06	1.72	10.95	1.75	.....	.....	11.03	10.88	2	.....	
Middle West	32.457	12.33	1.43	4.97	68.16	2.22	10.89	1.70	.....	.....	12.00	9.29	15	1	
Far West	37.528	9.50	1.55	5.30	70.75	2.47	10.43	1.67	52.679	28.348	12.78	8.23	53	53	
Pacific slope	27.900	9.78	1.48	6.40	72.13	2.07	8.14	1.30	.....	.....	8.40	7.88	2	1	
Mexico	.....	9.58	1.58	5.48	71.34	1.68	10.34	1.65	.....	.....	10.67	10.15	3	.....	
New Hampshire	.....	10.10	1.58	5.48	70.15	1.11	11.67	1.85	.....	.....	13.65	10.50	11	.....	Department of Agriculture.
Vermont	.....	8.64	1.45	5.63	72.76	1.38	10.14	1.62	.....	.....	.....	.....	1	.....	Do.
Connecticut	.....	10.84	1.54	4.70	70.98	1.46	10.48	1.69	.....	.....	11.63	9.19	13	.....	S.W. Johnson, W.O. Atwater.
Pennsylvania	37.320	7.96	1.43	5.22	74.54	1.97	8.88	1.42	44.147	30.610	10.15	7.53	5	3	Department of Agriculture.
North Carolina	.....	8.96	1.37	4.94	72.06	1.72	10.95	1.75	.....	.....	11.03	10.88	2	.....	Johnson and Dept.
Kentucky	32.457	7.70	1.50	5.33	73.47	2.20	9.80	1.57	.....	.....	.....	.....	1	.....	Department of Agriculture.
Tennessee	.....	7.58	1.23	5.09	74.16	2.65	9.29	1.48	.....	.....	.....	.....	1	.....	Do.
Indiana	.....	11.29	1.28	4.87	70.16	1.90	10.50	1.68	.....	.....	.....	.....	1	.....	R. C. Kedzie.
Michigan	.....	13.20	1.45	4.95	67.05	2.21	11.14	1.73	.....	.....	12.00	9.88	12	.....	Do.
Missouri	38.411	8.18	1.68	5.28	72.00	2.23	10.54	1.68	52.679	30.674	12.78	8.23	26	26	Department of Agriculture.
Kansas	33.271	11.94	1.49	4.87	69.39	2 16	10.15	1.62	36.687	28.348	10.68	9.10	6	6	Do.
Colorado	34.120	10.50	1.50	5.66	70.19	2.35	9.80	1.60	.....	.....	.....	.....	1	1	Do.
Texas	37.833	10.44	1.42	5.45	69.52	2.77	10.40	1.66	43.709	30.898	11.03	9.80	20	20	Do.
Oregon	.....	9.25	1.46	7.08	73.07	1.26	7.88	1.26	.....	.....	.....	.....	1	.....	Do.
Washington Terr.	27.900	10.30	1.50	5.73	71.19	2.88	8.40	1.34	.....	.....	.....	.....	1	.....	Do.
Mexico	.....	9.58	1.58	5.48	71.34	1.68	10.34	1.65	.....	.....	10.67	10.15	3	.....	Do.

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AVERAGE COMPOSITION OF AMERICAN WHEATS.

No. of analyses.		Weight of 100 grains.	Water.	Ash.	Oil.	Carbhydrates.	Fibre.	Albuminoids.	Nitrogen.	Heaviest 100 grains.	Lightest 100 grains.	Highest albuminoids.	Lowest albuminoids.
		Grams.	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent*	*Per cent.*	*Grams.*	*Grams.*	*Per ct.*	*Per ct.*
260	United States and Canada	3.638	10.27	1.84	2.16	71.98	1.80	11.95	1.91	5.924	1.830	17.15	8.05
108	Atlantic and Gulf States	3.464	10.42	1.75	2.17	72.01	1.72	11.33	1.81	5.079	1.830	15.58	9.45
47	The Middle West	3.607	10.51	1.76	2.01	71.67	1.90	12.15	1.94	4.902	2.138	16.63	10.15
97	West of the Mississippi	3.806	10.04	1.99	2.23	71.12	1.87	12.76	2.04	5.924	2.561	17.15	10.15
8	The Pacific coast	5.044	9.74	1.84	2.08	76.18	1.56	8.60	1.37	(5.745)	(4.253)	9.47	8.05
6	Canada	3.325	9.74	1 56	2.29	73.87	1.67	10.87	1 74	3.686	2.964	14.70	9.45
32	Pennsylvania	3.373	10.72	1.67	2.05	72.45	1.73	11.38	1.82	4.658	2.035	15.58	9.45
9	Maryland	3.597	10.52	1.75	2.09	72.25	1.74	11.65	1.80	5.079	3.075	14.53	9.80
11	Virginia	3.343	10.34	1.70	2.21	71.87	1.71	12.71	1.95	4.208	1.830	14.00	10.15
7	Georgia	3.579	10.00	1.96	2.30	72.24	1.73	11.78	1.89	4.627	2.834	14.00	9.45
22	North Carolina	3.776	10.03	1.59	2.25	73.94	1.76	10.43	1.67	4.628	2.780	12.43	8.93
17	Alabama	3.314	10.94	2.03	2.21	71.84	1.62	11.36	1.79	4.647	2.011	13.65	9.80
	Michigan (Kedzie)	.....	11.28	1.73		74.97		12.02	1.92	.....	.....	13.78	9.13
22	Michigan	3.969	10.71	1.64	2.06	73.12	1.80	11.67	1.87	4.902	3.402	15.23	10.50
8	Kentucky	3.454	10.83	1.75	1.87	70.37	2.03	13.15	2.10	3.666	3.146	14.53	11.90
14	Tennessee	3.150	10.19	1.89	2.00	71.33	2.02	12.51	2.00	3.990	2.138	16.63	10.15
12	Missouri	3.502	9.80	1.92	2.19	72.36	2.17	11.56	1.86	3.867	3.098	14.00	10.50
9	Minnesota	3.354	10.60	1.71	2.03	70.96	2.04	12.66	2.03	3.828	3.116	17.15	10.85
10	Kansas	3.204	11.80	1.64	1.98	71.35	2.08	11.15	1.78	3.424	2.881	12.25	10.50
19	Texas	2.847	10.03	1.81	2.11	70.85	2.06	13.14	2.10	3.937	2.561	15.23	10.68
45	Colorado	4.682	9.57	2.21	2.38	70.91	1.62	13.31	2.13	5.924	3.851	15.94	11.19
8	Oregon	5.044	9.74	1.84	2.08	76.18	1.56	8.60	1.37	5.745	4.253	9.47	8.05

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THE SOTOL, A MEXICAN FORAGE PLANT. By CLIFFORD RICHARDSON of Washington, D. C.

[ABSTRACT.]

THIS plant, *Dasylirion Texanum*, grows wild and extensively on the borders of the Rio Grande and elsewhere in Texas, and in Mexico, on a rocky and gravelly soil. The plains covered with it look like a vast cabbage-field. Sheep feeding on it go without water for many weeks. Only the bulb is eaten. It is split open by the shepherd, who carries a knife for the purpose. Mexicans eat the bulb after roasting or baking it in bits. Also a liquor is obtained from it, by fermenting and distilling after roasting, called "sotol mescal," and possessed of highly intoxicating powers.

The plant is described in Watson's Revision of the North-American Liliaceae. About 18 per cent of sugar can be obtained from the outer husks; in the interior, more than 10.5 per cent exists; and in the whole head of the plant there is probably more than 15.5 per cent of sugars. No starch seems to be present.

A proximate analysis of the soft interior of the head gave 17 per cent sugars: 62 per cent of this soft substance in the head, when fresh, is water.

As a food-plant in dry districts, the sotol is of great value; as a fibre-producing plant, it will not be of any importance, owing to the shortness of the cells.[1]

TWELVE MONTHS OF LYSIMETER RECORD AT THE NEW YORK AGRICULTURAL EXPERIMENT STATION. By E. L. STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

THE lysimeters were described. They are boxes of peculiar construction, containing the soil in its natural position. The rel-

[1] This paper will be printed in full in the Report of the U. S. Dept. of Agriculture.

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ative percolation of rainfall through these different soils, and the evaporation, are determined by observations of the instrument. The results are summarized as follows: sod land allowed 11.68 of the rainfall to percolate; soil of which the surface was simply bared allowed 25.88 per cent percolation; the cultivated soil passed 37.93 per cent. The evaporation from the first of these was, of course, 88.32 per cent; from the second, 74.12; from the third, 62.07; the sum of percolation and evaporation being held to account for the entire rainfall. This paper will be printed in substance in the Annual Report of the New York Agricultural Experimental Station for 1883.

NEW FORMS OF BURETTES. By W. H. SEAMAN of Washington, D. C.

ESTIMATION OF CARBON AND NITROGEN IN ORGANIC COMPOUNDS. By C. LEO MEES of Columbus, Ohio.

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SECTION D.

MECHANICAL SCIENCE.

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PAPERS READ.

Regularity of Flow in Double-cylinder Rotary Pumps. By J. Burkitt Webb of Ithaca, N. Y.

[ABSTRACT.]

The author introduced his subject by exhibiting a number of models of these pumps from the cabinets of Cornell University, which has recently purchased copies (248 in number) of the models of the celebrated Reuleaux collection in Berlin. Class I of this collection is devoted to these pumps. He then produced and demonstrated a formula for the flow of these pumps, and showed that the regularity of flow depends upon principles differing from those which have been given for determining this point. The formula given for the flow (for unity thickness) was:—

$$\pi\,[R'^2 + R''^2 - (r'^2 + r''^2)] =$$

flow for one revolution, where R' and R'' (generally equal to each other) are the extreme radii of the two revolving wheels, and r' and r'' are the radii (often, perhaps generally, variable) from the point of contact between the wheels to their centres. It was shown that for regularity of flow we must have $r'^2 + r''^2 =$ constant. R' and R'' may be called the "piston radii," and r' and r'' the "valve radii." These pumps are called by Reuleaux "Kapselräderwerke," or chamber-crank trains, according to Kennedy.

Improvements in Shaping-machines. By J. Burkitt Webb of Ithaca, N. Y.

[ABSTRACT.]

In the ordinary shaping-machine there are two defects, one of which is found also in the planer. The ram of a shaping-machine is a bar sliding in bearings, and carrying at one end the cutting-

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tool. If we represent by a the variable horizontal distance from the tool to the first bearing (or nearest end of the long bearing), and by b the variable horizontal distance from the tool to the second or farthest bearing (back end of long bearing), and by c the length of stroke, we shall have:—

$$\text{Maximum value of } a = (\text{minimum value of } a) + c$$
$$\text{Maximum value of } b = (\text{minimum value of } b) + c$$

In other words, the length of the ram is variable, and the spring of the ram from the work is variable, the tool springing away from its work more at the end of its stroke. This springing takes place mostly in the joint between the ram and its bearings, and cannot be wholly avoided without a change of construction. To remedy the defect, the author proposes a reversed construction of the sliding parts; the two bearings (preferable to a long bearing) to be formed on the ram, so as to make the distance a and b constant, and the long slide being part of the bed of the machine.

The second defect, which is also common to the planer, is in having a "drop-block" which fits but indifferently between the jaws and against the bottom of its seat. From the necessity of the usual construction, the tool attached to this block will have more or less spring. The remedy is to dispense with the drop-block, and introduce an automatic motion to lift the tool on the return stroke, as has been done, the author believes, on some large machines.

A Comparison of Terra-cotta Lumber with other Materials.

By Thos. R. Baker of Millersville, Pa

[ABSTRACT.]

The investigation which formed the subject of this paper was to ascertain certain properties of terra-cotta lumber. This material is a kind of potter's clay, and is prepared by mixing the clay with sawdust and then burning out the sawdust. The permeability of the material to air was determined by forcing air through it by means of a column of water. It was found to be 875 times as permeable to air as pine and 135 times as brick. Other tests showed that it is four times as hard as pine, and that its grip on nails driven into it is about half that of pine. It was regarded as a valuable article for the purposes for which it is intended.

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A METHOD OF TESTING LONG PLANE SURFACES, APPLICABLE TO THE ALIGNMENT OF PLANER-BEDS, LATHE-BEDS, HEAVY SHAFTING, ETC. By WM. A. ROGERS of Cambridge, Mass.

[ABSTRACT.]

THIS paper consisted of a statement of the difficulties of the problem, an account of four different methods tried by the writer, and the results obtained by the new method, which was described as "the method by two microscopes."

CENTRIFUGAL FORCE IN TURBINES. By R. H. THURSTON of Hoboken, N. J.

[ABSTRACT.]

A NOTE by Mr. J. P. Frizzell ("Journal of the Franklin Institute for June") especially interests the writer, as he has recently also noted the same defect in the accepted theory. It is so obvious as to require no proof that total centrifugal force, even where it is necessarily considered at all, depends for its measure upon the actual rotation of the mass of fluid, and not at all upon the movement of the wheel, except so far as the latter controls the former. In water-wheels, the two rotations are very different in their angular measures.

Plotting the path of the water in space, it will often be found that the line is so nearly rectilinear that the action of this force may be neglected as unimportant in practice. This is probably the case, for example, in the Vallet wheels. In the Whitelaw turbine, or "Scotch reaction wheel," as it is sometimes called, the spiral form given the arms may often be given such proportions that the path of the water may be nearly a straight line to the exit, and centrifugal force thus rendered so small that it may be neglected in the theory of that wheel. The writer has been interested in noting how much this method of treatment simplifies the theory of turbines, and how much more nearly, in some cases,

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the results accord with experiment. In the last form of wheel, for example, the expression for maximum efficiency becomes

$$E = \frac{1}{sec^2\,\theta},$$

and the speed of maximum efficiency becomes such that the orifice moves with the velocity due the head. The maximum effort is exerted, as it should be, when the wheel is held fast, and the effort becomes zero, when the speed of orifice becomes twice that velocity.

For the common case, in which the velocity of rotation of the water is a maximum at its entrance into the wheel from the guides, and becomes zero at exit from the wheel, varying in the interval by a uniform retardation, we easily produce expressions for the work done by or against the centrifugal force, and for the efficiency which will not here be given.

The Commercial and Dynamic Efficiencies of the Steam Engine.

By Robert H. Thurston of Hoboken, N. J

[ABSTRACT.]

The writer, at the meeting of 1882 of the American Association for the Advancement of Science, in a paper on a newly discovered "Absolute Limit to Expansion in the Steam Engine"[1] gave what he has called the "General Equation of Steam Engine Efficiency."

In that equation it was assumed that the annual expense chargeable to, or variable with, the cost of engine maintenance and operation could be reckoned per unit of volume of steam cylinder without serious error. Although this is often, and with large engines generally, true, and although it may not lead to appreciably erroneous results, it may yet be sometimes advisable to adopt the more general formula about to be given and which may be made as general and as exact as may be desired.

In the equation above referred to, an approximate form was adopted—making the denominator variable with variation of the

[1] Proceedings of the American Association for the Advancement of Science, 1882; Journal of the Franklin Institute, 1882.

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waste by cylinder condensation and leakage, instead of the second term in the numerator—for the purpose of obtaining a more manageable, although less precise expression.

The equation to be here given may be used when greater precision and a wider range of application is thought desirable than could be secured with the equation already given for general use in earlier papers.[2] The mathematical treatment may be found in the Franklin Institute Journal Sept., 1883, and by means of the analysis there given the "Maximum Efficiency of a Given Plant" can be easily determined.

VELOCITY OF A PISTON IN A CRANK ENGINE. By C. M. WOODWARD of Washington University, St. Louis, Mo.

[ABSTRACT.]

IN the case of a practically uniform angular motion of the crank, the maximum linear velocity of the piston is greater than the linear velocity of the crank pin. The points in the path of the pin, for which the piston velocity is a maximum, are 70° or 80° from the nearer dead-point, the exact position depending upon the relative length of the crank and connecting rod. This result was exhibited graphically and shown to be consistent with the obvious fact that more than half of the stroke is made when the crank has turned through 90° from the nearer dead point.

[2] Transactions of the American Society of Mechanical Engineers, 1882; Journal of the Franklin Institute, February to July, 1882.

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SECTION E.

GEOLOGY AND GEOGRAPHY.

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ADDRESS

BY

PROFESSOR C. H. HITCHCOCK,

VICE-PRESIDENT, SECTION E

THE EARLY HISTORY OF THE N. AMERICAN CONTINENT

There is a special appropriateness in the association of geography with geology, as indicated in the assignment of sciences to section E; for the latter gives us an account of the origin of every topographical feature of the earth's surface, whether island, continent, mountain, plateau, valley, or oceanic depression. If we would properly understand the significance of the earth's contours, we must unravel the mysteries of geology; so a knowledge of topography is essential to the complete comprehension of the geological features of any country. If a geologist were taken by a balloon to an unexplored part of the earth, he would instantly recognize, from their topographical outlines, volcanic and granitic cones, limestone hills, elevated plateaus of basalt or horizontal sandstones, and special types of orographic structure. Hence the modern geologist first draws the contours of his district before applying the colors of geological age. The existing relief features of the earth have been produced one by one in successive periods; and it is the task of the geologist to discover what were the characteristic physical developments of the several ages. He can delineate a connected historical sketch of the beginning, growth, and completion of a continent. Such histories are rare, because attention has but recently been turned into this direction. One of the first American geologists to frame such an outline is Prof. J. D. Dana, to whom we owe the enunciation of this fundamental truth,— that the first formed land has always remained above water, and has been a nucleus about which zones of sediment have accumulated. We can now recognize this primitive continent,

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with all its successive stages of growth, upon every geological map.

Time would fail us to present the entire physical history of our continent; and we will therefore confine our attention chiefly to its earlier chapters, noting those points which are under discussion. As we are endeavoring to *advance* science, we must touch upon debatable topics, and hope by friendly discussions to become wiser.

We must assume the correctness of the commonly received opinions concerning the earliest history of our planet,— that it passed through the condition of a nebula, and then of a burning sun, the period of igneous fluidity. By subsequent refrigeration it has become either partially or wholly solid. Not until a crust had formed, and the earth had cooled enough to allow water to remain permanently, was it possible to talk of dry land and ocean. With these premises allowed, it seems to us evident that the material of the earth must be disposed in concentric zones, arranged according to density, the heaviest being at the centre. If the various elements were free to move, as is the case in all natural or artificial igneous fluids, we must expect to find the heavier metals situated beneath the others; and, following the analogy of extra-terrestrial bodies, the central nucleus may be principally iron, like the heavier meteors. Zones corresponding to stony meteors, lavas, the trap family, and granites would naturally succeed in order, the last named being at the surface. This outer zone is also characterized by the presence of much silica and oxygen. The primeval ocean came from the vapors surrounding the igneous sphere, condensed to liquidity as soon as water could remain upon the solid crust without immediate vaporization.

This original crust may have been essentially a plain, and consequently entirely covered by water; for if the land were now levelled off, the ocean would submerge every acre of the continents. As refrigeration progressed, ridges and valleys would form in accordance with that fundamental principle that the outer envelope must conform to the shrunken nucleus; and this contraction gives rise to that tangential force or lateral pressure which has acted through all time. Whether these earliest ridges rose above the ocean would depend upon the amount of elevation. Some authors argue that these ridges follow the course of great circles. If there are causes adequate to produce such results,— or any other world-wide arrangement,— they must have commenced to operate

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at the very beginning of contraction. Most authors maintain that the very thick strata of the older rocks have been formed just like modern sediments, having been broken off the ledges, and transported into oceanic basins in horizontal attitude. If so, there must have been great mountainous elevations, deep oceanic depressions, and extensive aqueous action; since the thickness of the crystalline schists is greater than that of the strata in the fossiliferous ages. The amount of distortion, crumpling, and faulting of the crystalline rocks is also greater. These same authors hold that the original strata were in all respects like modern sands, gravels, and clays, and that their present crystalline structure is due to metamorphism. No one has yet discovered any uncrystalline pre-Cambrian beds; nor have the original foundation rocks been pointed out, since the oldest known layers are stratified, and cannot therefore have constituted part of the original unstratified crust.

Professor Dana thinks the primitive land originated because of a difference in the rate of conduction of heat during the process of refrigeration. Cooling would be fastest where the heat was conducted most rapidly. The first areas to cool would be the first to solidify. The first solidified crust was heavier than the adjacent liquid; so it sank until it found a fluid as dense as itself. Then the liquid above this crust would in turn become solid, and sink; and this process is supposed to have continued until a permanent shell had become fixed in the earth's circumference, which constantly increased in breadth and thickness, becoming continents. Meanwhile the other portions remained liquid; and their surfaces must have stood at the same level with the first-formed crust till that congealed, and became depressed because of the diminution of volume in solidification. These depressions became the ocean's beds. From this beginning down to the present time the processes of growth have consisted in the thickening of the continents and the settling-down of the oceanic depressions, while the chief force employed has been the lateral pressure derived from contraction. LeConte and Pratt express the process thus far described by the term "unequal radial contraction." The total gravity of the particles of matter along each radius is supposed to be the same; and hence, if the heat is conducted most rapidly over the shorter radii composed of denser minerals the ocean-basins would cool first. These two views thus demand a different

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arrangement of the lighter and denser materials, as pointed out by Mr. W. O. Crosby; the one necessitating that the continents, and the other the depressions, were first to congeal. Both, however, make the gratuitous and unproved assumption, that the surface was not uniform in composition; the difference being probably like that between granite and trap. The principle stated above — that, where all the particles are free to move in a liquid, the lighter elements must rise to the surface, and the heavier minerals sink down in proportion to their specific gravity — is at variance with this assumption. Fortunately it is not essential to a right theory of continental growth. There is no reason, therefore, to doubt that the original crust had essentially a uniform thickness over the whole earth. Contraction would originate ridges and valleys in the normal way, most likely of similar dimensions. There must soon burst forth ejections of igneous matter, owing to tidal attraction; and these would show themselves along the weakest lines. At the outset it is difficult to assign reasons why either the elevations or depressions would be the weaker; and hence we should look for a multitude of locations of igneous overflow, both over the future continents and oceans. There may be no better reason for the eventual enlargement of certain of these volcanoes than that circumstances only very slightly favored them; but, this favor being continued, they would exist and enlarge at the expense of the others, affording us another illustration of the "survival of the fittest."

It seems to us there is now afforded an opportunity for reviving in a modified form the view of Poulett Scrope in regard to the origination of the earlier crystalline deposits. Suppose we say, that, besides the original unstratified igneous granitic material, the oldest stratified crystalline rocks are derived from volcanic ejections; being the continued enlargement in size, and reduction in number, of the early indeterminate vents. The several ejections would increase in size till they became islands, either gneissic or granitic; and, if an archipelago is allowed us, we can easily show how continents would accumulate, using only the universally acting forces of lateral pressure and sedimentary accumulation.

Of other theories relating to the origin of the earlier crystalline beds I may mention two. The first is that advocated by Lyell, who termed these rocks *hypogene*. After the solid granitic crust had been formed by refrigeration, "the hot waters of the ocean

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held in solution the ingredients of gneiss, mica-schist, hornblende-schist, clay slate, and marble,— rocks which were precipitated one after the other in a crystalline form" (Lyell's Principles of Geology, tenth ed., i, 142). In such a menstruum, life could not have existed. A very similar view was advocated by Dr. T. Sterry Hunt in his presidential address before this association in 1871.

The second is the view more commonly entertained,— that, after the solidification of the crust, sedimentation accumulated stratified systems from the granitic foundations, as ordinary sand, gravel, and clay, which were subsequently acted upon by thermal and aqueous influences, termed *metamorphic*, and thus converted into crystalline schists. The widespread and powerful action of metamorphism is conceded; but it is a more appropriate adjunct to volcanic than sedimentary accumulation.

A few of the considerations favoring our theory will now be presented.

1. Considering the igneous origin of the earth, volcanic energies would naturally continue their action as soon as there was a crust to be broken through, and immense piles of melted rock would ooze from the numerous fissures. Up to Laurentian times all admit the universality of igneous outflow, while but few have ventured to speak of anything like volcanic action, except as it has been manifested in the formation of dikes in these early periods. There has been a tendency to class the ancient granites and porphyries with rocks of sedimentary origin, and consequently to restrict the action of igneous agencies to phenomena of slight importance. Several English writers, and, in our country, Dr. Selwyn of Canada, have been calling our attention to the existence of a volcanic group in later Huronian or early Cambrian times. These are the rocks so largely developed about Lake Superior, New England, and the Province of Quebec, consisting of stratified schists, diorites, diabases, amygdaloids, and felsites, identical in composition with true eruptive masses of the same name. Investigation shows that oftentimes these schists are disposed like the lavas ejected from one series of volcanic vents. Suppose, for example, that Etna or Vesuvius should become extinct. In the course of ages the rains would obliterate the craters, and reduce the lavas to a rounded dome of greater or less regularity. We should recognize the volcanic origin of the mountain in the absence of craters, from the lithological similarity of the rocks to those

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known to have been melted and ejected from vents, while the disposal of the material in a conical attitude shows us that it might once have been covered by craters. So we find in our eastern country many domes of diabasic or protogenic schists, whose volcanic origin may be predicated, both from their lithological character and physical aspect.

Now, this volcanic group of Huronian times indicates the existence of a greater degree of igneous activity than has been described for the paleozoic ages, even in Great Britain; and consequently this is an indication pointing significantly towards the predominance of thermal influences in the still earlier periods. In the Laurentian age the fires should have been yet more vigorous, because the time of universal igneous fluidity was less remote.

2. A careful study of the crystalline rocks of the Atlantic slope indicates the presence of scattered ovoidal areas of Laurentian gneisses. Those best known have been described in the Geology of New Hampshire. Instead of a few large synclinal troughs filled to great depths with sediments, the oldest group is disposed in no less than twenty-two areas of small size, scattered like the islands in an archipelago. In a chapter upon the physical history of the state, I have proposed the theory that the earliest land within its limits consisted of this series of islands, not packed together as closely as now, in an area of perhaps three thousand five hundred square miles, but as much more widely separated as would be determined by smoothing out the various anticlinals and synclinals that were formed later. By reference to our maps in Maine, Massachusetts, New Jersey, Pennsylvania, Virginia, North Carolina, and Georgia, many similar ovoidal Laurentian areas may be specified, usually larger than those of New Hampshire. This may be due partly to a less thorough knowledge of the exact areas occupied by this older gneiss, and partly to the existence of a greater number of volcanic vents, giving rise to a more widely spread and thicker mass of ejected material. Over the Atlantic slope and Canadian Highlands these primeval islands have, in later periods, been cemented together by a subsequent deposition of material; but in Missouri, Arkansas and Texas, we recognize, even now, these early islands.

3. The lithology of the Laurentian and other crystalline rocks is very like that of igneous ejections. It is proper at this point

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to recall the proper signification of the term Laurentian. As originally defined by Logan, it included every formation that antedated the Huronian. The subdivisions of lower, middle and upper Laurentian are often used by the Canadians. Logan spoke of the gneiss of Ottawa and of Trembling mountain as being decidedly older than most of the series. In his report for 1845 he distinguished the groups by the absence of limestone in the lower and its presence in the upper series. This distinction was again alluded to by him in 1857 but never made prominent afterwards. Several years later Logan defined an upper series, characterized by the presence of triclinic feldspars, and it was regarded as overlying unconformably the gneisses holding limestone. The names Labrador and Norian have been applied to this upper series. Inasmuch as these upper rocks are largely eruptive, Selwyn and others think that the institution of a Labrador system is a mistake, and hence that the lower and middle. or the Ottawa and Grenville series represent the whole of the Laurentian.

In the report upon the geology of Canada for 1877–1878, Selwyn proposes to restrict the Laurentian outcrops to "all those clearly lower, unconformable, granitoid, or syenitic gneisses in which we never find interstratified bands of calcareous, argillaceous, arenaceous, and conglomeratic rocks." The Hastings and Grenville series, and all the schists containing the eozoon, are excluded from the Laurentian by this definition, as well as the Bethlehem, Lake Winnipiseogee, and Montalban groups of the Atlantic slope. The lower Laurentian is azoic, the other groups eozoic; and, unless newer distinctions are to be made hereafter, it looks as if we might claim these various azoic Laurentian islands as the first-formed dry land, as they certainly are the nuclei of the existing continents.

There are no minerals in these Laurentian islands that do not occur in eruptive granite; and the schistose structure is often so faint that the field geologist need not be blamed if he acknowledges his inability to detect it. Likewise we discover the same fluidal inclusions and the vacuoles that pertain to granite. If we follow Sorby and Clifton Ward in saying that granite has been formed beneath a pressure equivalent to a weight of forty thousand feet of strata, the same must be said of the early gneisses. With this general assertion of the identity of gneiss and eruptive granite, we must be satisfied at present, without entering into detail.

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4. The analogy of the origin of oceanic islands at the present day suggests the igneous derivation of the Laurentian areas. Most of the high islands of the Pacific are composed of lava, with the volcanoes frequently in action.

The so-called lowlands are likewise of volcanic origin; since coral polyps have built up reefs upon the igneous area after the disappearance of the fire, and the Hawaiian areas are encircled by reefs. After the volcanoes have become cold, loose material would be worked in between them, coral reefs would grow, and, in various ways, the land area would be enlarged, and finally an archipelago may become a large island. It needs only time and a repetition of these constructive agencies to make a continent out of a series of archipelagoes.

5. The oceanic volcanic islands are not inferior in size to the crystalline Laurentian areas upon the present continents. Hawaii, of the Hawaiian group, may illustrate their position and shape. Its area above the water-line is 4,210 square miles, and its cubical contents above the sea-level are about the same with those of New Hampshire. It rises from a plateau over 16,000 feet deep, thus forming a cone 30,000 feet high, whose cubical contents must be twenty times greater than the portion making dry land. The length of the entire series of islands, all of similar character, is 350 miles, and the area of the base of the lava must be about 100,000 square miles. These cones have been built up by the accumulation of lava ejected from the interior of the earth, and they are entirely isolated, the nearest land being 1,000 miles distant. The ground-plan of this volcanic mass is that of two elliptical areas, either of which is like some of our Laurentian islands, and is certainly as large as any of these ancient lands south of the St. Lawrence. The land area of the Hawaiian Islands is less than that of Massachusetts, but their base must be equal to the whole of New England and New York combined. Surely it cannot be avowed that volcanic areas are too small to be compared with the space occupied by our oldest formation.

There are two points requiring explanation in this connection, — first, the supposed deeply seated localities where granite is produced; and, second, the origin of foliation in the schists.

We should naturally expect that the earlier igneous rocks would have been derived from reservoirs quite near the surface, because of the thinness of the crust. With this notion agrees the presence

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of cavities containing liquid, and of hydrated minerals, which are more common in the older eruptive rocks, and have led to the aqueo-igneous theories of the origin of granite. Water would be scarce at great depths, and hence these rocks ought to originate near the surface where moisture was abundant. It seems to us that this consideration should more than balance the arguments usually cited in favor of the origin of granite at enormous depths, as it is difficult to see how both can be true.

Mr. H. C. Sorby has led the way in studies of the mineral constituents of eruptive rocks. He measures the included cavities in the component minerals, and calculates how much the contained substances must have contracted in cooling, allowing for an increase in the temperature of the point of vaporization under pressure. By assuming the temperature to be correctly determined, he ascertains the amount of pressure indicated by mathematical formulæ, and finds it to be the equivalent of a thickness of 40,000 feet of overlying rock in Cornish granites, and of 60,000 feet in Scotch granites. Later writers seem to have regarded this pressure as certainly produced in the way thus suggested, and that its appearance at the surface has been due to an enormous erosion which has denuded the overlying blanket. This conclusion is not necessary; for, *first*, an enormous pressure would result from the tangential force of contraction, which would be entirely adequate to have produced the cavities. *Second*, the necessity of an erosion of 40,000 feet over all the granites in every part of the world cannot be maintained. In North America, for example, it would necessitate the supposition that nearly eight miles' thickness of rock had been removed from one-fourth of the surface since the Laurentian, for the blanket removed would have equalled in dimensions the crystalline areas. The mere statement of the amount of denudation required refutes the theory. *Third*, by reference to existing volcanoes, it is plain that a column of lava will often be adequate to exert the needed pressure. Teneriffe rises 12,000 feet above the ocean, and its cone descends 18,000 feet more to the submarine plateau. When the crater is full of melted lava, there must be a pressure of 30,000 feet at the base of the cone: hence the lava from the reservoir supplying Teneriffe might exhibit the vacuities produced by a pressure of 30,000 feet without any weight above the peak.

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When molten lava pours down the side of a crater, the included vapors and liquids must disappear because of the removal of the pressure, but, after a substantial crust has formed, the peculiar markings imprinted at the great depth would remain: hence we can understand how it is that the vacuities are to be seen both in granites and lavas that have been subjected to great pressure. At the Boston meeting of this association I endeavored to show that there are mountain masses of granite in New England possessing all the physical characteristics of volcanic cones. The material must have been liquid, hot, ejected from a vent, and flowed over a plateau, building up a cone, and indurating the underlying floor. It was claimed that such phenomena could be explained only by supposing the granite to have been erupted just like lava. This granite contained the usual vacuities indicative of great pressure just as they are also found in the lava of Monte Somma or the trachyte of Ponza.

When one examines the interior structure of modern lava-flows, he is surprised to find beds nearly as well defined as the foliation of schists. Around vents like Vesuvius or Etna the lava accumulates naturally in quaquaversal sheets, no one eruption being very extensive. When steam and hot water are copiously supplied from the caldron, there may be flows of hot mud and tufa. The closing phases of eruptions are usually showers of ashes falling upon the cone or beyond. If the vent is beneath the ocean-level, the lava is minutely subdivided and the deposit will be like sand or gravel. Between the igneous flows the ordinary aqueous agencies will wear off excrescences, and scatter the fragments down the slope. These various agencies will produce a concentric stratiform arrangement in the whole mass. Where the eruption is massive, a similar set of layers will be formed.

This mass of volcanic material will be very susceptible to metamorphic influences when placed under the proper conditions of heat and pressure. As the result, new minerals will be formed, arranged in foliated beds or schists. Thus briefly stated may be the origin of foliation. So long ago as 1825, Poulett Scrope advocated essentially this doctrine for the arrangement of the crystalline particles in the crystalline schists, having found an analogous structure in certain volcanic accumulations.

Sufficient has now been said in advocacy of our doctrine that

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the first land consisted of volcanic islands. This was the Laurentian or azoic accumulation. Cartographers have not yet distinguished the several crystalline deposits, so that it will not be practicable at present to point out the supposed volcanic areas of the Hastings, Grenville, Montalban, Huronian, and other eozoic periods. Sedimentation would also act so that in this age many beds must be referred to an aqueous derivation. By the close of the eozoic age the continent was outlined; or at least the framework of the future superstructure was put into position. The broader patches about to be mentioned had their origin in the earlier numerous islands cemented by detrital accumulations.

The more important areas developed in the eozoic must have been Greenland, Canada east of Lake Winnipeg, the Atlantic district, the Rocky Mountains, the Sierra Nevadas, and numerous buttes over the Cordilleras. The three great depressions of Hudson's Bay, the Mississippi valley, and the Salt-Lake and Nevada basins commenced to sink very early, and the future growth of the continent consisted largely in filling them up with marine sediments. An inspection of a map drawn upon a correct scale will dissipate the fancied resemblance to the letter V, in the Canadian dominion, so often insisted upon. Neither has the development of the land been in bands parallel to the northwest and southeast arms of this supposed angle. A better conception would find three great basins, excluding the unknown regions of Mexico and Alaska, in each of which operations were conducted independently. The best known is that of the interior of the United States, or the Mississippi hydrographic basin. This depression was nearly encircled by a crystalline border of high land. Beginning at Alabama, we follow it to New England, thence by a slight gap to the Adirondack promontory, thence across the Lakes to the Dakota promontory. In Minnesota and Dakota the schists are more or less covered by cretaceous clays and tertiary sands; but they evidently constitute the floor for the later deposits, as seen in the Black Hills. Thus we may connect the Dakota and Rocky Mountain crystallines. From Wyoming southerly the granites are again conspicuous into New Mexico. Thus the circuit is not complete: it is like a horseshoe, with the lower Mississippi valley in the gap; yet this may have been filled in the Cambrian age, since Laurentian islands are found in Texas, Arkansas, and Missouri.

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We might give reasons for believing in the recent origin of the depression between New Mexico and Alabama.

The map will show, around the borders of this Mediterranean Sea, the primordial sea-beach, whether examined in Virginia, New York, Michigan, Colorado, or Texas. Could we dissect the land, we should find an immense platter of Cambrian sediments coëxtensive with the crystalline highlands surrounding and underlying it. In Cambro-Silurian times the story is repeated. Marine limestones formed other dishes, each limited in size by the upturned edges of the platter underneath. The rest of the history is given in our text-books. Our Mediterranean Sea was not closed till the end of the cretaceous, when the salt-water was expelled, never to return.

In the west a similar ovoidal, crystalline border can be traced, holding paleozoic sediments. Beginning at the Rocky Mountain chain in Wyoming, we follow it southerly to Mexico. Across Arizona are many gneissic outliers, but not sufficiently numerous to close the gap. In California we reach a country entirely gneissic beneath the sands of the desert, which connects with the Sierra Nevadas, and is traceable along the Nevada line nearly to Oregon. There the course is changed, the rocks trend northeasterly, show themselves conspicuously in the Blue Mountains of eastern Oregon, the Salmon River Mountains of Idaho, and western spurs of the Rockies again in Montana, which are continuous to our starting-point in Wyoming. Our crystallines do not pass north of the parallel of 49° into Columbia. We have therefore found a complete crystalline border for the depressions of our western territories, and, within this ovoidal line, all the members of the paleozoic, mesozoic, and cenozoic groups, but not arranged with the simplicity of their distribution in the east.

Less is known of the arctic basin than of the others; but the scattered sketches afforded by voyagers indicate the presence of the more important members of the geological column. Where these basins adjoin, there is a much wider area of ancient land.

In conclusion, I will simply recapitulate the more important phases of the growth of our continent.

We start with the earth in the condition of igneous fluidity.

It cools so as to become incrusted and covered by an ocean.

Numerous volcanoes discharge melted rock, building up ovoidal

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piles of granite, which change gradually into crystalline schists. When these hills are high enough to overlook the water, they constitute the beginnings of dry land.

At the commencement of paleozoic time the continent is composed of three immense basins, one located near Hudson's Bay, the Mississippi hydrographic area, and the great Nevada series of land-locked valleys.

The later history of the development of the continent presents the details of the filling-up of these depressions, the expulsion of the Mediterranean seas, and the description of the varied forms of life that successively peopled the land and water.

The history opens with igneous agency in the ascendant. Aqueous and organic forces became conspicuous later on, and ice has put on the finishing touches to the terrestrial contours. The completed structure we must acknowledge to be "very good."

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PAPERS READ.

EVIDENCE FROM SOUTHERN NEW ENGLAND AGAINST THE ICEBERG THEORY OF THE DRIFT. By JAMES D. DANA of New Haven, Conn.

IN presenting to the Association evidence from southern New England with regard to the insufficiency of the iceberg theory of the drift, I shall have to say some things that have often been said before, and by various investigators. But I may claim for what is here brought forward, that it is, in my own mind, the fortified conclusion of long-continued investigation.

The arguments on the subject are derived from three sources:—

I. The scratches and groovings over the rocks.
II. The transported bowlders and other materials.
III. The facts as to the relative level of the land and sea.

I. The scratches or grooves over the rocks.

Under this head there is, first, the old argument based on the universal distribution of the scratches over the region of all New England. These effects of abrasion are to be found everywhere beneath the soil, each fresh exposure of the rocks bringing them to light. This was said years ago; and the conviction of its truth has been gaining force with every year of additional observation.

a. In view of this fact, it is urged rightly that only an abrading agent that pressed heavily against the broad rocky surface could have produced the effects; and such is not an occasionally grounding iceberg, or a succession of them. Neither is it the still more locally acting shore-ice.

b. Floating ice would have found little bare rock over the sea-bottom to be abraded. Like the bottom of existing seas, and eminently those of the continental borders, the submerged region would have had for the most part a bottom of detritus, its former detritus, and additional detritus from later depositions. The

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removals would have been local, and relatively of small area. Consequently, the drifting ice would rarely have reached down to the rocks. Shore-ice carried along by the currents would have had a better chance, and yet a poor one, for the work to be done.

c. The character of the groovings and ploughings is, to a great extent, such as floating ice could not have produced. As has been often said, the close uniformity of direction and parallelism over large areas, which so generally prevails, is not a possible result of iceberg action. The needed pressure and steadiness of movement are wanting.

Troughs in hard granite even six inches deep are the work of one and the same moving tool for a long period; and one year would be long for the steady action of an iceberg. If grounded, it would do almost nothing; if floating free, absolutely nothing; and a nice adjustment to depth would be required for any steady abrasion, much nicer than would have long continued anywhere over the uneven bottom.

In the Triassic sandstone of East Haven, Conn. (just east of New Haven), at a place where the sandstone is a very firm, thick-bedded, gritty rock, the ploughing ice ploughed out a piece of moulding, somewhat like the *ogee* of the carpenter, which was 8 feet deep, 25 feet wide, and over 120 feet long, and perfectly even in surface as well as direction.

d. The currents that would have borne along the icebergs over submerged New England, in case of a submergence sufficient to cover the highest striated surfaces,—3,000 to 5,500 feet,—would have been those of the present ocean, the Labrador current, and Gulf stream; and, with less submergence, the same in part, modified by the courses of the valleys and the tides. It is to be noted, that the New Haven region, in Connecticut, is the southern extremity of the Connecticut valley. The mean trend of this valley in Connecticut is about S. 15° W., and, in southern Connecticut, S. 18° W. Now, the numerous scratches over the *eastern* portion of the New Haven region average in direction S. 16° W.; but along its *western* border, where the rapidly rising slopes give the region rather an abrupt limit 150 to 350 feet high, the scratches have an average course of S. 33° W., the extreme being S. 27° W., and S. 55° W.; and S. 33° W. is the almost uniform trend over the undulating surface of the country for six to nine miles west. It is, as far as I can see, impossible that the valley stream should have

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had on its west side so wide a divergence from the direction of the Connecticut valley: all the features of the region oppose it. The scratches are well exposed over the metamorphic rocks in many places; and large and perfect examples of *roches moutonnées* here occur.

Again: over the higher lands of western Connecticut (and of New England generally, according to the observations of Prof. Edward Hitchcock, Prof. C. H. Hitchcock, and others), the direction of the scratches is southeastward. To have produced them, if icebergs were the agent, the submergence should have exceeded 2,500 feet, and this would have given a chance for the full play of the oceanic currents; and yet the above direction does not correspond with that of either of the great currents.

II. Distribution of the drift.

Bowlders of trap, from 50 to 1,000 tons in weight, are numerous in the New Haven region, especially along its western border. All are Connecticut-valley travellers; for the trap ridges of the valley—400 to 1,300 feet in height—are the only possible source. They were gathered up by the ice from these trap ridges, and were carried 15 to 60 miles down the valley. It is mechanically impossible that the larger bowlders should have been taken up, or gathered in any way, by floating ice; either shore-ice, where the water was but 1,000 feet deep and less, or by that of icebergs, where the depth was greater. It is well known, that the distance of drift transportation is in general less than 20 miles. Hills of but 100 feet often have their long trails. A moving glacier would easily gather and carry along the material from hills, high or low, wherever loose or detachable masses of rock or gravel existed to be gathered; while floating ice would be very poor at gathering, and hence inefficient in distributing.

III. Relative level of the land and sea.

I have examined carefully along southern New England for proofs of the Quaternary submergence which the iceberg theory assumes to have existed in the glacial era. I thought at one time that I saw evidence about New Haven of a submergence of 45 to 50 feet. But the terrace that afforded the evidence was situated six miles back from Long Island Sound, adjoining the rivers; and on further examination I found that the deposits had

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precisely the structure of those along the river-valleys farther north, and that, in fact, they were nothing but fluvial formations. The highest terraces on or near the shores of the sound, in the vicinity of New Haven, have a height above mean tide of 23 to 26 feet; and on Milford bay, nine miles west, a similarly situated terrace has a height of 30 to 33 feet. Along the hills facing the waters, and the southern extremity of the valleys, no traces of any higher level exist. Twenty-five to thirty-five feet is the greatest amount of submergence the facts sustain. Seaborder deposits exist at a higher level on the coast of Maine and on the shores of the St. Lawrence, and show what was the position of the shore-line in those regions. But the level along southern New England is not proved by the facts there gathered, neither is it established by the demands of the iceberg theory.

In conclusion, if icebergs, or floating masses of ice, were not capable of covering with scratches great continuous areas, and would have had a chance for little rock-abrasion on account of the covering of detritus; if they could not have made, in their hitching and swinging way of action, when touching bottom, scratches over great areas, that had the even course and parallelism characterizing those of drift regions, or could not have ploughed out the deep furrows; if they could not have gathered the great bowlders for transportation; and if the sea along the sound did not cover the land, in any part of the era of ice, to a greater depth than 30 or 35 feet,—the iceberg theory of the drift may be reasonably pronounced unsatisfactory for southern New England; and similar facts show that it is equally unsatisfactory for the rest of New England.

THE ANCIENT GLACIATION OF NORTH AMERICA: ITS EXTENT, CHARACTER, AND TEACHINGS. By J. S. NEWBERRY of New York.

[ABSTRACT.]

WHILE the glacial area on our continent has not been fully explored, there is abundant proof for the following propositions: 1. Glaciers covered most of the elevated portions of the mountain belts in the west as far south as the 36th parallel, and in the

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eastern half of the continent to the 40th parallel of latitude. 2. The ancient glaciers, which occupied the area above described, were not produced by local causes, but were evidences of a general climatic condition. 3. They could not have been the effect of a warm climate and an abundant precipitation of moisture, but were results of a general depression of temperature.

The traces of glaciation are similar in kind, and apparently in date, over the whole area: they are therefore effects of general, not of local, causes. East of the Mississippi, the evidence is even more widespread and impressive than in the far west. The area bearing marks of ice action, and strewn with drift, extends from New England westward, parallel with the Canadian highlands, in a belt five hundred miles wide and over two thousand miles long. Its northern extension has not been traced beyond Winnipeg; but there are reasons for believing that it reached to the Arctic ocean, and that the great lakes are preglacial river-valleys, scooped out and modified by ice. Fully half the continent north of the 36th parallel was glaciated. So far as we now know, the glaciation was synchronous.

The iceberg theory was opposed on the following grounds: it postulated a water-line with irregularities of level that are irreconcilable. The direction of the scratches, and the lines of derivation of the bowlders, require that the northern portion of the continent should have been all submerged, leaving no land for the origin and starting-point of icebergs. If icebergs could have been formed and floated, an incomprehensible tangle of ocean-currents would be required to account for their movements. The evidence of sea-covering, in the form of marine shells, is totally absent from the great drift area of the interior, while they are found abundantly in the Champlain and bowlder clays of the coast. Finally, the inscription left by the eroding agency is characteristic and *sui generis*.

The record of the ice period on our continent is far more extensive and impressive than has been represented. The phenomena were due to extraneous and cosmical causes, not to anything local or even telluric.

The question here passes from the geologist, and must be addressed to the astronomer. The author briefly recapitulated some of the theories which have been suggested by Croll, Newcomb, and others, to account for the glacial epoch.

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THE ERODING POWER OF ICE. By J. S. NEWBERRY of New York, N. Y.

[ABSTRACT.]

THE object of this essay was to enter a protest against the theories of certain geologists who claim that glacial ice has not played an important part in the erosion. Several heresies have been advanced in regard to this subject: 1. The denial that there ever was a glacial period. 2. The assertion that if there was an ice period it was a warm and not a cold one. 3. That the phenomena usually ascribed to glacial action in the record of the ice period were generally due to icebergs. 4. That ice has little or no eroding power, and that glaciers have never been important geological agents.

In refutation of these theories the author gave the result of his extended study of glacial action in the Alps, and in many different parts of the United States and Canada. The conclusions to which he had been led by his observations were: 1. That the glacial period was a reality, and that its record constitutes one of the most striking and interesting chapters of geological history. 2. That this was a cold period. 3. That ice has great eroding power, and that in regions which they have occupied, glaciers have always been important and often preponderating agents in effecting topographical changes.

The facts on which these conclusions were based may be summarized as follows: 1. All the mountain chains and highlands of the west have been simultaneously covered with ice or snow fields, for moraines, excavated lake-basins, rock-surfaces marked with the peculiar inscription of glaciers, rounded hillocks, hills and mountains attest the former presence of ice in the Rocky Mountain belt, in the Wasatch and in the Sierra Nevada from the 36th parallel as far northward in British America as explorations have been carried. 2. On the Atlantic coast the glacial topography, glaciated rock-surfaces and glacial debris (till, moraines, kames and Champlain clays), stretch continuously from New York to Maine, and, as we learn from many observers, to Greenland, where continental glaciers covering thousands of square miles and apparently some thousands of feet in thickness are doing the work of which the record is left farther south. 3. The highlands north of the St. Lawrence, once an elevated mountain range, have been

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planed down to an undulating surface, the topography resulting from ice action, and everywhere showing ice marks. 4. A belt of country parallel with the Canadian Highlands reaching from Maine west and northwest to and beyond Lake Winnipeg, an area 2000 miles by 500, is glaciated and covered with glacial debris to an average depth of 30 to 50 feet. 5. The basins of the Great Lakes were certainly once occupied by glaciers, which moved approximately in the direction of their longest diameters, and have been in part, at least, excavated by ice action. 6. Similar records reported by Forbes and Whitmell in Norway, Ramsay, Geikie and others in the British Islands, Penck in Bohemia, Agassiz, Guyot and others in Switzerland, Godwin-Austen in India, Darwin and Agassiz in South America, confirm the testimony of the rocks here, and establish beyond question at least one ice period as a marked chapter in geological history, and prove the enormous eroding power of ice.

The erosion of ice is effected by sand, gravel, and bowlders frozen into its under surface and carried forward in its motion; where the glaciers were 1000 feet in thickness the grinding material was pressed upon the underlying rock with a force of 48,700 pounds to the square foot. The ancient glaciers of New England were from 3000 to 5000 feet in thickness, exerting a pressure of from 150,000 to 250,000 pounds to the square foot. The weight, grinding material and incessant motion, make glaciers over the area they occupy far more powerful eroding agents than water. This is proved by the relative amounts of sediment carried by the streams which drain glaciers and those which do not. The latter are made turbid only by occasional and temporary rains, while the former are always opaque from the quantity of sediment they transport; and yet this is only the finer material ground up by the glacier, the coarse being left behind. The quantity of transported matter has in some instances been measured: for example, the stream which drains the Aar glacier brings down 280 tons per day, and the Justedal glacier of Norway wears down 69,000 cubic metres of solid rock annually; and these are measurements of the eroding power of two small glaciers

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THE LIFE-HISTORY OF THE NIAGARA RIVER. By JULIUS POHLMAN of Buffalo, N. Y.

[ABSTRACT.]

IN preglacial times the valley of the Tonawanda was occupied by a lake whose waters found their way into the Ontario valley across the upper rapids at Niagara Falls, thence along the present river gorge down to the whirlpool, and from here by the way of the buried St. Davis channel. After the ice period this outlet was closed. Lakes Erie and Ontario drained simultaneously, as testified by beaches around both lakes, until both stood on a level with the Lewiston heights. From this time Lake Ontario drained slowly with many resting places and the waters of Lake Erie flowed over the mudflat along the preglacial river valley between Buffalo and Lewiston. We must therefore reject any theory of falls at Lewiston; contrary to hitherto accepted data, the waters could, at their best, form only a rapid over the escarpment; for the Niagara limestone here is only thin, and it was a comparatively easy task for the immense volume of water to excavate the soft strata of rock between Lewiston and the whirlpool, where it met the bed of the preglacial river up to the present site of the falls. This fact explains why the river turns almost at right angles opposite the whirlpool. I therefore conclude that the time necessary to excavate the gorge of the Niagara below the falls has always been overestimated, for that portion which would take the longest time, between whirlpool and fall, was already excavated in preglacial times and the three miles between whirlpool and Lewiston were composed almost entirely of shales.

RESULT OF EXPLORATIONS OF THE GLACIAL BOUNDARY BETWEEN NEW JERSEY AND ILLINOIS. By G. F. WRIGHT of Oberlin, Ohio.

[ABSTRACT.]

AFTER citing reasons for desiring a careful *résumé* of the subject, — the observations being scattered in the works of different explorers,— the author proceeded to name those who had determined, for different regions, the southern boundary of the glaciated area.

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Starting at the eastern coast, President Edward Hitchcock was the first to intimate that the backbone of Cape Cod was a part of the terminal moraine if the theory of Professor Agassiz were true. Clarence King made a similar assertion as to accumulations near Wood's Holl and on the Elizabeth islands. Professor Charles H. Hitchcock declared that the backbone of Long Island was the foot of a terminal moraine. Warren Upham went over this field, from the end of Cape Cod to Brooklyn, to verify the hypothesis. Professors Cook and Smock traced the moraine across the state of New Jersey. Professor Lesley commissioned Professor H. Carvill Lewis and the author of the paper to continue the exploration across Pennsylvania. In Ohio, Professor Newberry had approximately outlined the boundary; but in Ohio, Indiana, and Illinois, the survey was carried on by a number of different persons before the most distinctive glacial features were fully understood.

The chief indications on which reliance can be placed to determine glacial action are striated rocks, striated stones, bowlders, and till. Rocks near the margin are often so deeply covered with till that their markings are not apparent. The softer rocks do not always retain their striæ; this has often been the case in Ohio. In certain situations, stones might be striated by a landslide, or the grounding of an iceberg; but the area over which striated stones are found is too vast for such explanation of their presence. The bowlders are of granite and metamorphosed rocks from northern Canada and the shores of Lake Superior; their presence is relied upon only when they are in such situations as to preclude the likelihood of their having been transported by the agency of rivers. Till is spread over the whole area: it is defined as an unstratified deposit, containing striated stones of various sizes,— fragments of rock foreign to the locality. Its composition varies, through mixture with underlying material. It covers and gives fertility to northern Ohio, Indiana, and Illinois. Till has been well characterized by Professor Newberry as the grist of the glacier.

Briefly told, the boundary-line of the glaciated area, so far as now accurately known, is as follows: beginning on the island of Nantucket, it runs through Martha's Vineyard, No Man's Land, Long Island from east to west, across Staten Island, entering New Jersey at Perth Amboy, and after bending north-

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ward and making a right angle near Dover, crosses the Delaware at Belvidere. Thence it runs northwesterly through Northampton, Monroe, Luzerne, Columbia, Lycoming, Tioga, and Potter counties in Pennsylvania, and Cattaraugus county, New York, reaching its most northerly point about six miles north of Salamanca. From here it runs through Warren, Venango, Butler, Lawrence, and Beaver counties, to the Ohio line, crossing Beaver creek at Chewtown about fifteen miles above the Ohio river.

The boundary enters Ohio in the northern part of Columbiana county, and proceeds nearly west to the middle of Stark, then turns more to the south, touching the corner of Tuscarawas, and dividing Holmes into two nearly north-and-south sections. Near the northeast corner of Knox, the line makes a right angle, and runs south through Knox, Licking, the northwest corner of Perry, Fairfield, Ross, Highland, Adams, and Brown counties. Then it follows the line of the Ohio river across Clermont, and enters Kentucky near the boundary between Pendleton and Campbell counties, and, after crossing the northern part of Kenton and Boone counties, recrosses the Ohio, entering Indiana a little below Aurora.

In Indiana the line still continues to bear in a southerly direction through Ohio and Jefferson counties, grazing the edge of Kentucky again opposite Madison, and reaching its southernmost point near Charlestown in Clarke county, Ind. From here it bears again to the north, through Scott and Jackson counties, to the line between Bartholomew and Brown, and follows this to the northeast corner of Brown. There again it turns to the southwest, touching the northeast corner of Munroe, where it again bears north for ten miles, to near Martinsville in Morgan county. Here again the line turns to the west and south, passing diagonally through Owen, Green, Gibson and Knox counties, and in Posey as far as New Harmony. Beyond this point, the line has not been accurately traced.

The signs of glaciation cease where there is no barrier to account for their cessation, and where no barrier ever could have existed such as must be supposed if the so-called glacial phenomena are the product of floating ice. Of the correctness of this inference, the different elevations at which the signs of glacial action cease

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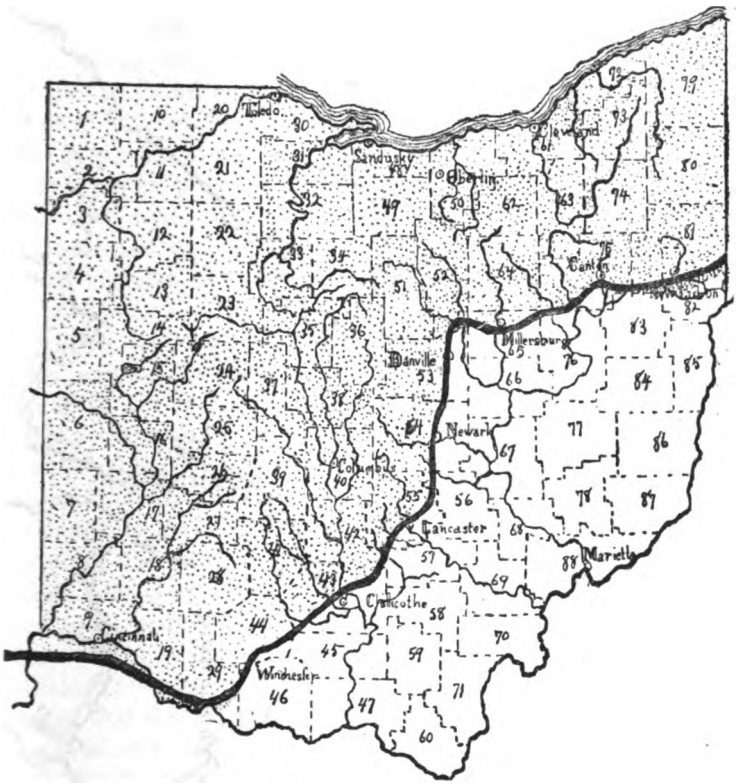
Map showing Southern Boundary of Glaciated Area of Ohio. [From G. F. Wright's Report to the Historical Society of Cleveland, Ohio.]



LIST OF COUNTIES WITH NUMBERS CORRESPONDING TO THOSE IN MAP.

1. Williams
2. Defiance
3. Paulding
4. Van Wert
5. Mercer
6. Darke
7. Preble
8. Butler
9. Hamilton
10. Fulton
11. Henry
12. Putnam
13. Allen
14. Auglaize
15. Shelby
16. Miami
17. Montgomery
18. Warren
19. Clermont
20. Lucas
21. Wood
22. Hancock
23. Hardin
24. Logan
25. Champaign
26. Clarke
27. Greene
28. Clinton
29. Brown
30. Ottawa
31. Sandusky
32. Seneca
33. Wyandot
34. Crawford
35. Marion
36. Morrow
37. Union
38. Delaware
39. Madison
40. Franklin
41. Fayette
42. Pickaway
43. Rose
44. Highland
45. Pike
46. Adams
47. Scioto
48. Erie
49. Huron
50. Lorain
51. Richland
52. Ashland
53. Knox
54. Licking
55. Fairfield
56. Perry
57. Hocking
58. Vinton
59. Jackson
60. Lawrence
61. Cuyahoga
62. Medina
63. Summit
64. Wayne
65. Holmes
66. Coshocton
67. Muskingum
68. Morgan
69. Athens
70. Meigs
71. Gallia
72. Lake
73. Geauga
74. Portage
75. Stark
76. Tuscarawas
77. Guernsey
78. Noble
79. Ashtabula
80. Trumbull
81. Mahoney
82. Columbiana
83. Carroll
84. Harrison
85. Jefferson
86. Belmont
87. Monroe
88. Washington.

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Southern Boundary of Glaciated Area in Indiana. [From G. F. Wright's Report to the Hist. Soc. of Cleveland, Ohio.]

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are sufficient proof. For instance, the line is near sea-level in New Jersey; in Pennsylvania it rises over Mount Kittatinny to a height of 1,600 feet, then descends 800 feet into a valley, and, again rising, reaches the summits of mountains 2,000 feet above sea-level. Crossing the valley of the Susquehanna at an elevation of only 500 feet, the line mounts the Alleghenies diagonally, and runs over them at a height of 2,100 feet.

The paper proceeds to describe certain marked features of glaciated areas. South of New England, the terminal line is characterized by a series of glacial hills, 100 to 300 feet high. These are also observable in New Jersey, near Plainfield and Menlo Park.

Among the most interesting results of the author's survey in Ohio, was the demonstration of the existence of an ice-dam across the river at Cincinnati. The line bounding the glacial accumulations crosses the Ohio river into Kentucky near the boundary between Campbell and Pendleton counties, about twenty-five miles above Cincinnati, and recrosses it near Aurora, Ind., about twenty-five miles below Cincinnati, thus filling the channel for about fifty miles of its course. The Ohio, it should be said, occupies, throughout nearly its whole extent, a narrow valley of erosion, not often more than a mile wide, and from 300 to 500 feet deep. Emptying into the main channel there are subordinate channels all along, of smaller dimensions, but of nearly equal depth. The proofs that the ice bodily crossed the river at the points indicated are, that till and granitic bowlders are found on the Kentucky hills south of the river to a certain distance, and not beyond it.

To the question, Why is the boundary of the glaciated area so crooked? the author replied at some length; assigning as a principal cause, aside from differences of level, the probability that unequal amounts of snow fell over different regions to the north. The effect of such differences of accumulating snowfall, in determining the extension of the glacial outline, is illustrated by supposing that two loads of sand are placed in one pile, and one load in an adjoining pile, when the sand will flow downward to unequal distances upon a level.

A little reflection will show that the glacial theory will not make extravagant suppositions as to the amount of ice required. The ice was indeed 6000 feet deep over New England, and, very

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likely, of an equal depth over the area to the west; but it is not necessary to suppose a great increase of this depth to the north. All that is necessary is to suppose great accumulations of ice to the north of the granitic hills of Canada, starting a movement past them to the south. This movement may have been kept up toward the margin by fresh accumulations of snow upon the spreading glacier. An accumulation of snow over the glacier in any part of it would spend its effective force in giving impetus to the movement of the front along the lines of least resistance.

GENETIC CLASSIFICATION OF THE STONY DRIFT CLAYS. By T. C. CHAMBERLIN of Beloit, Wis.

[ABSTRACT.]

IN a somewhat extensive study of the drift east of the Rocky mountains, the importance of a more complete genetic classification of the drift had been felt by the speaker. In so far as it is possible to do so, it is important to determine the origin of any given drift deposit by its *inherent* characteristics, for in the closer distinctions which the progress of this department of science now demands, stratigraphical and other extraneous characters often fail to present decisive criteria. This is especially true of the pebble and bowlder clays. Certain of them seem to be plainly of glacial formation, while others are as certainly to be assigned a different origin. The discrimination between them on the basis of inherent constitution is one of extreme difficulty, and only partial success can be expected, but the attempt is none the less important.

1. One characteristic difference between the classes of drift clays is the manner in which pebbles and bowlders are embedded in them. In subaqueous clays, lines of lamination, in some portions of the deposit, at least, are to be expected, and these are often seen to be depressed beneath the enclosed pebbles or bowlders, as if the latter had been dropped upon a yielding surface of clay,

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while the laminæ above curve over the erratics conformably to them, indicating subsequent deposition. This is regarded as a decisive criterion, though not universally applicable, since all subaqueous drift is not distinctly laminated.

2. Again in subaqueous clays, the pebbles and bowlders are found to be commonly in that position in which they would be likely to strike on falling through water upon soft clay. In the true glacial clays, on the contrary, the rock fragments have been observed to have no definite or uniform position. This again, where applicable, is an important discrimination.

3. The distribution of bowlders through the two clays is another means of discrimination. In subaqueous clays the bowlders are observed to be distributed with such uniformity as the law of hazard would prescribe. In the glacial clays proper, on the other hand, there is a manifest tendency to a clustering of the bowlders. Currents would of course modify the distribution of bowlders in subaqueous clays. Other agencies also might operate to effect the same result, but the general lack of uniformity of distribution of bowlders through the one class and of clustering through the other is of wide application.

4. The surface contours of the glacial and subaqueous clays afford a still further means of distinction. The latter are, in general, characterized by plane topography, while the former generally present several distinct phases of irregularity. Aside from the peculiar surface aspects of the moraine ridges, the lenticular or elliptical hills present a distinct type of glacial accumulation. Allied to these two diverse classes, there are two prevalent systems of linear ridging of drift, the one parallel to glacial movement and the other transverse to it; the former, perhaps due to the influence of preëxisting irregularities of surface and of rock-resistance, determining a tendency to flow in channels, the latter to secular and seasonal advances of the ice. Both of them grade away into unclassifiable irregularities. Other means of discrimination, less specific, or less capable of brief statement, may be found; but combining the testimony of all, there are still certain pebbly clays that as yet elude positive identification by means of their inherent natures, whatever judgment of them may be based upon external considerations.

The gradations of one type into another present grave difficultics.

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The Character of the Outer Border of the Drift. By T. C. Chamberlin of Beloit, Wis.

[ABSTRACT.]

West of the Scioto valley in Ohio the extreme outer border of the drift is not usually marked, so far as the writer's observations have extended, by any definite moraine, applying that term only to ridged aggregations of drift due to the forcible action of glacier ice. East of that point on or near the margin, definite morainic ridges prevail. Outside of these is the so-called glacial fringe, a border of thin drift or scattered bowlders.

In the western interior, however, an analogous phenomenon attains a much more important development. The drift margin here graduates away to a vanishing edge. In some portions, especially in Dakota, bowlders constitute almost the entire portion positively referable to drift. These repose upon residuary material of local origin. This border of thin discontinuous drift reaches its greatest extension in Dakota, west of the Missouri river. In this region glacial clay and gravel are quite limited in quantity, the peripheral drift consisting very largely of archæan bowlders, there being but few of palæozoic limestone. These bowlders are notably clustered, the groups being gathered mainly on prominences, the intermediate depressions and even many of the eminences being comparatively free from them. The clusters lie at unequal altitudes and repose sometimes on summits and sometimes on slopes. West of the Missouri river there is little or no evidence of glacial ploughing, the topography being mainly of the drainage type. East of the river, in general, the erosion topography is noticeably subdued. The Missouri does not, however, accurately mark the transition from the one type to the other.

West of Pierre there are but few bowlders, but on the east bluffs there is an extraordinary accumulation of them. At other points, where studied, the proximate border was found marked by an unusual accumulation of bowlders, beyond which there was a scattered distribution for a considerable distance.

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THE TERMINAL MORAINES OF THE LATER EPOCH. By T. C. CHAMBERLIN of Beloit, Wis.

[ABSTRACT.]

THE marginal accumulations thus far discussed mark the extreme limit of glaciation, and, in the interior regions, belong to the earlier glacial epoch.

Turning to the border of the later drift, we may recall that in 1873, an investigation of a peculiar line of drift-hills in eastern Wisconsin, locally known as the Potash Kettle Range, which it had been suggested represented an old beach line, was undertaken.

Investigation soon developed evidence, that, instead of a beach line, the range is a moraine. The region affords exceptional advantages for securing decisive data, and results essentially demonstrative are attainable. These show that the Kettle Range is an interlobate moraine formed jointly by glacial lobes occupying the valleys of Lake Michigan and Green Bay respectively. It was found that this moraine bifurcates in southern Wisconsin, one portion continuing southward, nearly parallel to the shore of Lake Michigan, while the other curves to the west and then north forming a broad loop outlining the glacial tongue that then occupied the Green-bay-Rock-river valley.

From this initial region the observations of the writer have extended along these two branches eastward and westward, and, in connection with the work of others, have shown an essentially continuous morainic chain to the Atlantic on the one hand, and the British Possessions on the other. The looped disposal of the moraine, accompanied by reëntrant angles and interlobate projections, signifying an extremely lobed ice margin, and divergent internal ice-movements, has been found to prevail throughout the interior. The distribution of these great loops was traced by the writer upon the map, and their significant relations to the great valleys indicated. The irregularities of the development of the moraine were referred to and associated with topographical relations.

In response to interrogation as to the time relations between the older and the newer drifts, Professor Chamberlin stated that one of the best bases of estimation was to be found in the amounts of erosion suffered by the regions of older and younger drifts respectively. Attention was called to the fact that the lakes are almost entirely confined to the region north of the terminal mo-

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raine of the second epoch. Professor Chamberlin regards the erosion suffered by the older drift area as fully twice that of the region north of the terminal moraines of the second epoch.

Incidentally in the discussion, the theory recently advanced by Jamieson that the larger lake basins are due *in part* to a depression of the crust caused by the accumulation of the ice was endorsed qualifiedly. Dr. Chamberlin had, previous to a knowledge of Mr. Jamieson's views, gathered data pointing to the same conclusion. Preglacial erosion had done much to form these basins and glacial corrasion something additional; but neither of these, nor both combined, are thought to furnish an adequate explanation of the phenomena presented. An additional cause of some potency is believed to be found in the influence of ice occupancy. In the discussion following, exception was taken to this hypothesis of depression caused by ice accumulation. The principle, however, is regarded by Dr. Chamberlin as one of wide application, depression being caused by accumulations of sediment in regions of detrital deposit, as well as by ice. This affords an easy explanation of the subsidences which have accompanied—*pari passu*—great detrital accumulations.

The basins of the Great Lakes are regarded as due to the joint agencies of preglacial erosion, glacial corrasion, glacial accumulations, blocking-up outlets, depressions due to ice occupancy and general crust movements, together with possible unascertained agencies.

RELATION OF THE GLACIAL DAM AT CINCINNATI TO THE TERRACES IN THE UPPER OHIO AND ITS TRIBUTARIES. By I. C. WHITE of Morgantown, W. Va.

[ABSTRACT.]

THIS paper, in the absence of its author, was read by Professor Winchell.

In a paper read before the Boston Society of Natural History, March 7, 1883, Rev. G. F. Wright showed that the southern rim of the great northern ice-sheet covered the Ohio river near the site of New Richmond, a few miles above Cincinnati; and presented the hypothesis, that one effect of this invasion of the Ohio valley

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by the glacial ice was to form an immense dam of ice and morainic *débris*, which effectually closed the old channel way, and set back the water of the Ohio and its tributaries until, rising to the level of the Licking River divide, it probably found an outlet through Kentucky, around the glacial dam. .The writer of the essay, after reviewing the evidence, regards Mr. Wright's hypothesis as proved beyond a reasonable doubt. He also claimed, that during the period of the continuance of the dam, the principal tributaries of the Ohio had their valleys filled with sediment carried down and dumped into them by the mountain torrents and other streams which drained the area south from the glaciated region; that, subsequently, when the barrier disappeared the rivers recut their channels through the silt deposits, probably by spasmodic lowering of the dam, in such a manner as to leave the deposits in a series of more or less regular terraces, which in favored localities subsequent erosion has failed to obliterate, though from steep slopes it has removed their every trace. The elevation of this dam at Cincinnati, as determined from the upper limit of the fifth Monongahela River terrace, would be somewhere about 625 feet above low water there in the present Ohio.

In discussing this paper, Professor Lesley said there were two separate glacial formations to be considered, and the two could not be correlated. The ice-dam accounted perfectly for all the facts with regard to the terraces along the Monongahela, and he sustained Professor Wright's conclusions.

THE MINNESOTA VALLEY IN THE ICE AGE. By WARREN UPHAM of Minneapolis, Minn.

THIS essay is based upon my observations as assistant on the geological and natural history survey of Minnesota, from 1879 to the present time, under the direction of Prof. N. H. Winchell, the state geologist. Some portions of it have been before published in the annual reports, and others are here presented in advance from manuscripts prepared for the final reports of this survey.

The Minnesota river, from which this state is named, has its highest and most remote sources upon the Coteau des Prairies, about 2000 feet above the sea. Its head-stream, after flowing

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eastward twenty miles, turns southerly at Brown's Valley, and enters the northwest end of Big Stone lake. Here and in its whole extent thence to its mouth, the Minnesota river occupies a very remarkable valley, the origin of which was first explained in 1868 by Gen. G. K. Warren, who attributed it to the outflow from an ancient lake that filled the basin of the Red river and Lake Winnipeg. This valley or channel begins at the northern part of Lake Traverse, and first extends southwest to the head of this lake, thence southeast to Mankato, and next north and northeast to the Mississippi at Fort Snelling, its length being about two hundred and fifty miles. Its width varies from one to four miles, and its depth is from one hundred to two hundred and twenty-five feet. The country through which it lies, as far as Carver, about twenty-five miles above its junction with the Mississippi, is a nearly level expanse of till, only moderately undulating, with no prominent hills or notable depressions, excepting this deep channel and those formed by its tributary streams. Below Carver it intersects a belt of terminal moraine, composed of hilly till. Its entire course is through a region of unmodified drift, which has no exposures of solid rock upon its surface.

Bluffs in slopes from twenty to forty degrees, and rising one hundred to two hundred feet to the general level of the country, form the sides of this trough-like valley. They have been produced by the washing away of their base, leaving the upper portion to fall down and thus take its steep slopes. The river in deepening its channel has been constantly changing its course, so that its current has been turned alternately against the opposite sides of its valley, at some time undermining every portion of them. In a few places this process is still going forward, but mainly the course of the Minnesota river is in the bottomland. Comparatively little excavation has been done by the present river. As we approach its source it dwindles to a small stream flowing through long lakes, and we finally pass to Lake Traverse, which empties northward; yet along the upper Minnesota and at the divide between this and the Red river, this valley or channel and its enclosing bluffs are as remarkable as along the lower part of the Minnesota river. It is thus clearly shown to have been the channel of outflow from a lake formerly extending northward from Lake Traverse along the Red river valley. The existence of this body of water is believed to have been due to the barrier

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of the receding continental ice-sheet, obstructing the natural course of drainage northward as at present to Hudson bay; and it is therefore called *Lake Agassiz*, in memory of Professor Louis Agassiz, the first prominent advocate of the theory that the drift was produced by land-ice. The shore-line of this lake, when it stood at its highest level, marked by a distinct beach of gravel and sand, upon a surface which on each side of this beach is chiefly till, has been traced continuously 175 miles, from Lake Traverse easterly to Herman and thence northerly to Maple lake, twenty miles east-southeast from Crookston. The height of this beach has been ascertained at the same time, by levelling along this entire distance.

The Minnesota valley in many places cuts through the sheet of drift and reaches the underlying rocks, which have frequent exposures along its entire course below Big Stone lake. This excavation shows that the thickness of the general drift-sheet upon this part of Minnesota averages about one hundred and fifty feet. The contour of the old rocks thus brought into view is much more uneven than that of the drift. In the hundred miles from Big Stone lake to Fort Ridgely the strata are metamorphic gneisses and granites, which often fill the entire valley, one to two miles wide, rising in a profusion of knolls and hills, 50 to 100 feet above the river. The depth eroded has been limited here by the presence of these rocks, among which the river flows in a winding course, crossing them at many places in rapids or falls. From New Ulm to its mouth the river is at many places bordered by Cretaceous and Lower Silurian rocks, which are nearly level in stratification. These vary in height from a few feet to 50 or rarely 75 or 100 feet above the river. From Mankota to Ottawa the river occupies a valley cut in Shakopee limestone underlain by Jordan sandstone, which form frequent bluffs upon both sides, 50 to 75 feet high. After excavating the overlying 125 to 150 feet of till, the river here found a former valley, eroded by preglacial streams. Its bordering walls of rock, varying from one-fourth of a mile to at least two miles apart, are in many portions of this distance concealed by drift, which alone forms one or both sides of the valley. The next point at which the river is seen to be enclosed by rock-walls is in its last two miles, where it flows between bluffs of Trenton limestone underlain by St. Peter sandstone, 100 feet high, and about a mile apart. This also is a preglacial channel, its

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further continuation being occupied by the Mississippi river. The only erosion effected by the Minnesota river here has been to clear away a part of the drift with which the valley was filled. Its depth at some earlier time was much greater than now, as shown by the salt-well on the bottomland of the Minnesota river at Belle Plaine, where 202 feet of stratified gravel, sand and clay were penetrated before reaching the rock. The bottom of the preglacial channel there is thus at least 165 feet lower than the mouth of the Minnesota river.

Heights of the bluffs, which form the sides of the valley, composed of till, enclosing layers of gravel and sand in some places, and frequently having rock at their base, are as follows, stated in feet above the lakes and river: along Lake Traverse, 100 to 125; at Brown's Valley and along Big Stone lake, mainly about 125, the highest portions reaching 150; at Ortonville, 130; at Lac qui Parle and Montevideo, 100; at Granite Falls, 150; at Minnesota Falls, 165; thence to Redwood Falls, Fort Ridgly and New Ulm, 165 to 180; at Mankato, 200 to 225; at Saint Peter and Ottawa, 220 to 230; at Le Sueur and Henderson, 210 to 225; at Belle Plaine and Jordan, about 230; and at Shakopee, 210 to 220. The morainic hills through which this valley extends below Shakopee are 225 to 250 feet in height. The expanse of till through which this channel is eroded slopes from 1125 feet above the sea at Big Stone lake to 975 at Mankato, in 140 miles; and thence it descends to 925 at Shakopee, in 50 miles. This channel, or valley, of the Minnesota river, lies nearly midway between the belt, on its northeast side, of medial and terminal moraines, that extends from Lake Minnetonka 150 miles northwest to the Leaf hills, and the Coteau des Prairies on its southwest side; toward each of which, some fifty miles distant from this river, there is a gentle ascent, sufficient to cause drainage to follow this central line.

The height of Lake Traverse is 970 feet above the sea; the lowest point in Brown's valley between this and Big Stone lake is only three feet above Lake Traverse; Big Stone lake is 962 feet above the sea, or eight feet below Lake Traverse; and the mouth of the Minnesota river is 690 feet above the sea, the descent from Big Stone lake to the mouth of the river being 272 feet.

Lakes Traverse and Big Stone are from one to one and a half miles wide, mainly occupying the entire area between the bases

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of the Bluffs, which rise about one hundred and twenty-five feet above them. Lake Traverse is fifteen miles long; it is mostly less than ten feet deep, and its greatest depth probably does not reach twenty feet. Big Stone lake is twenty-six miles long, and its greatest depth is reported to be from fifteen to thirty feet. The portion of the channel between these lakes is widely known as Brown's valley. As we stand upon the bluffs here, looking down on these long and narrow lakes in their trough-like valley, which extends across the five miles between them, where the basins of Hudson bay and the Gulf of Mexico are now divided, we have nearly the picture which was presented when the melting ice-sheet of British America was pouring its floods along this hollow. Then the entire extent of the valley was doubtless filled every summer by a river which covered all the present areas of flood-plain, in many places occupying as great width as these lakes.

Gen. Warren observed that Lake Traverse is due to partial silting up of the channel since the outflow from the Red river basin ceased, the Minnesota river at the south having brought in sufficient alluvium to form a dam; while Big Stone lake is similarly referred to the sediment brought into the valley just below it by the Whetstone river. Fifteen miles below Big Stone lake, the Minnesota river flows through a marshy lake four miles long and about a mile wide. This may be due to the accumulation of alluvium brought into the valley by the Pomme de Terre river, which has its mouth about two miles below. Twenty-five miles from Big Stone lake, the river enters Lac qui Parle, which extends eight miles, with a width varying from one-fourth to three-fourths of a mile and a maximum depth of twelve feet. This lake, as Gen. Warren suggested, has been formed by a barrier of stratified sand and silt which the Lac qui Parle river has thrown across the valley. He also showed that Lake Pepin on the Mississippi is dammed in the same way by the sediment of the Chippewa river; and that Lake St. Croix and the last thirty miles of the Minnesota river are similarly held as level back-water by the recent deposits of the Mississippi.

All the tributaries of the Minnesota river have cut deeply into the drift, because the main valley has given them the requisite slope. The largest of these extend many miles, and have their mouths level with the bottomland of the Minnesota river. The

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bluffs of all these valleys are also everywhere seamed and gullied by frequent rills and springs, many of which flow only after rains. Few of the large inlets have any great amount of sediment deposited opposite their mouths, showing that their excavation was mostly done at the same time with that of the main valley. The short ravines are more recent in their origin, and the material that filled their place is commonly spread in fan-shaped, moderately sloping banks below their mouths, which are kept at a height from 30 to 40 feet above the present flood-plain. The road from Fort Ridgely to New Ulm runs along the side of the bluff at the only height where a nearly level straight course could be obtained, being just above these deposits and below the ravines.

The valleys of the Pomme de Terre and Chippewa rivers, 75 to 100 feet deep along most of their course, and one-fourth of a mile to one mile in width, were probably avenues of drainage from the melting ice-fields in their northward retreat. Between these rivers, in the twenty-two miles from Appleton to Montevideo, the glacial floods at first flowed in several channels, which are excavated 40 to 80 feet below the general level of the drift-sheet, and vary from an eighth to a half of a mile in width. One of these, starting from the bend of the Pomme de Terre river, one and a half miles east of Appleton, extends fifteen miles southeast to the Chippewa river near the centre of Tunsburg. This old channel is joined at Milan station by another, which branches off from the Minnesota valley, running four miles east-southeast; it is also joined at the northwest corner of Tunsburg by a very notable channel which extends eastward from the middle of Lac qui Parle. The latter channel, and its continuation in the old Pomme de Terre valley to the Chippewa river, are excavated nearly as deep as the channel occupied by the Minnesota river. Its west portion holds a marsh generally known as the "Big slough." Lac qui Parle would have to be raised only a few feet to turn it through this deserted valley. The only other localities where we have proof that the outflow from Lake Agassiz had more than one channel are seven and ten miles below Big Stone Lake, where isolated remnants of the general sheet of till occur south of Odessa station and again three miles southeast. Each of these former islands is about a mile long, and rises 75 feet above the surrounding lowland, or nearly as high as the bluffs enclosing

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the valley, which here measures four miles across, having a greater width than at any other point.

Terraces and high plains of modified drift, found in many places along the valley of the Minnesota river from New Ulm to its mouth, show that it was once filled, doubtless at the close of the last glacial epoch, with stratified gravel, sand and clay, to a depth 75 to 150 feet above the present river. The remnants of this deposit include the plateau of modified drift, about a mile long and an eighth of a mile wide, upon which the west and highest part of New Ulm is built; a terrace in section 27, Courtland, opposite the southeast part of New Ulm, more than a mile long and about an eighth of a mile wide; a larger terrace, four miles long and a half mile wide, lying also in Courtland, four to eight miles southeast from New Ulm, upon which Courtland depot is situated; a terrace extending about three miles northwest from near Minneopa falls, and varying from a few rods to a third of a mile in width; a terrace three miles long east and south of Kasota; the "sand prairie," about four miles long and averaging a mile wide, west and north of Saint Peter; Le Sueur prairie, six miles long and from one to three miles wide, beginning east of Ottawa and reaching to Le Sueur; the plain five miles long and a mile wide, near the middle of which Belle Plaine is built; Spirit hill and "sand prairie," southwest and northwest of Jordan; a terrace eight miles long and varying from a few rods to two miles in width, extending through San Francisco, Dahlgren, and Carver; and Shakopee prairie, eight miles long and averaging one mile wide. The height of these terraces and plains at New Ulm is about 115 feet above the river; in Courtland, and near Minneopa falls, 125 to 150 feet; at Kasota, Saint Peter and Le Sueur, about 150 feet; at Belle Plaine, about 135; and at Jordan, Carver and Shakopee, about 125. Wells on the "sand prairie" near Saint Peter and on Le Sueur prairie go through sand and gravel, sometimes with layers of clay, to the depth of 75 or 100 feet, finding till below. At Belle Plaine the sand and gravel are about 50 feet deep, underlain by till. Shakopee prairie has 40 or 50 feet of this modified drift, lying upon limestone. The principal remnant of these deposits seen below Shakopee was a terrace about 75 feet high, an eighth to a third of a mile wide and four miles long, extending through Eagan in Dakota county, its north end being about two miles south of Fort Snelling. This valley was first excavated in

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till, which rises in continuous bluffs on each side 50 to 100 feet above these high plains and terraces of modified drift. It was afterward filled along this distance of one hundred miles next to its mouth with fluvial deposits 75 to 150 feet thick, sloping about two feet per mile, through which the channel has been cut anew.

With this general view of the valley of the Minnesota river as it exists to-day we are prepared to inquire more particularly what its history was during the vicissitudes of the ice age and from the close of that period until now. Its condition at the beginning of the reign of ice first claims consideration. During what ages was its preglacial rock-walled channel formed? This is in part answered by deposits of Cretaceous clay found in waterworn hollows of the Shakopee limestone forming the walls of this channel at numerous places in Blue Earth, Le Sueur and Nicollet counties, and by the occurrence in this valley in Courtland and New Ulm and near Fort Ridgely and Redwood Falls, of Cretaceous sandstone, clay and shale, occasionally containing lignite. It is thus known that before the Cretaceous age, when western Minnesota and the region of the upper Missouri were depressed and covered by the sea, a deep channel had been cut by some river in the Lower Magnesian strata of the Minnesota valley. The slopes from each side toward this channel appear therefore to have been partly like those of the present day. The direction of drainage may even have been the same as now, for the northern part of the Mississippi river and valley has probably existed since the middle of Palæozoic time.

Scanty exposures of Cretaceous strata are found in many parts of Minnesota, enclosing sometimes marine fossils, sometimes impressions of leaves, and at a few places thin layers of lignite. The western two-thirds of the state were probably covered until the glacial period by deposits of this age which have now been mainly eroded, with much from the underlying Palæozoic rocks, and constitute a part of the drift, irrecognizably mingled with detritus and boulders that have been brought from Laurentian and Huronian areas far to the north and northeast. Excepting its partial submergence by the sea in the Cretaceous age, Minnesota seems to have stood wholly above the level of the ocean from the beginning of the Carboniferous period till the present day.

Before the ice age the rocks had been long subjected to the ordinary disintegrating agencies of rain and frost. Granite and

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gneiss were thus generally decomposed in place to considerable depths. The loose material resulting from such decay was doubtless spread somewhat evenly over the surface, collecting to the greatest depth in valleys. Except where it had been transported by streams and consequently formed stratified deposits, the only fragments of rock held in this mass would be from underlying or adjoining rocks.

During the ice age the Cretaceous strata, mostly unconsolidated, which covered western Minnesota, the superficial deposits and decomposed rock produced by weathering, and the alluvial beds of sand and gravel along the river-courses, were mostly ploughed up by the ice and thoroughly kneaded with each other. Much detritus was also added from erosion of the harder rocks beneath. Large blocks and boulders, often already formed by the process of weathering, were borne away, and the surface of the bed-rock was worn and striated by boulders and pebbles, which were rolled and dragged along under the vast weight of ice, breaking up and grinding themselves and the underlying rock into gravel, sand, and even the finest clay.

Ledges of decomposed gneiss and granite, found in the Minnesota valley at many points from Minnesota Falls to Fort Ridgely, with their surface changed to a soft, earthy or clayey mass, resembling kaolin, and the Cretaceous beds before mentioned, both of which would readily yield to eroding agencies, show that the moving ice-sheet did not everywhere plough up all the loose material under it. A considerable depth, however, has probably been removed; and these may be scanty remnants of thick beds which covered this region generally before the glacial period. More commonly the ice-sheet removed all such material, and gathered a part of its drift from the underlying solid rocks; as is shown by their being frequently rounded, smoothed, and marked with parallel furrows and scratches, called striæ. Similarly scratched pebbles and boulders are found in the glacial drift. These were the graving tools by which the bed-rock was worn and striated. They were held firmly by being frozen in the bottom of the ice and were pushed forward by its current, which thus recorded its direction. Our observations of glacial striæ in the Minnesota valley are as follows: one to three miles southeast from the foot of Big Stone lake, S. E.; near Odessa, S. E.; at Granite Falls, in several places, S. 45°–50° E.; at the dam of O K mill, Beaver

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Falls, S. 60° E.; one and a half miles west from Fort Ridgely, S. 60° E.; at Redstone, one and a half miles southeast from New Ulm, S. 25° E.; at Jordan, noted by Foss, Wells & Co., in quarrying and on the site of their mill, S. E.

Within the till are frequently found layers of sand or gravel, which yield the large supplies of water so often struck in digging wells. Probably many of these veins of modified drift were formed by small subglacial streams and therefore cannot be regarded as marking divisions of the glacial period, nor even any important changes in the overlying ice. It appears, however, by shells, remains of vegetation, and trees, found deeply buried between glacial deposits in this and adjoining states, that the ice age was not one unbroken reign of ice, but that this retreated and readvanced, or was possibly at some times nearly all melted upon the northern hemisphere and then accumulated anew. Thus periods of ice alternated with interglacial epochs, in which animal and vegetable life spread again northward, following close upon the retreat of the ice-fields. By each new advance of the glacial sheet much of the previous surface would be ploughed up and redeposited; hence we find only few and scanty remnants of fossiliferous beds in the glacial drift. At the disappearance of the last ice-sheet these drifted materials, seldom modified by water in their deposition, formed a mantle 100 to 200 feet thick, which throughout the basin of the Minnesota river almost universally covered the older rocks.

Two principal glacial epochs can be especially distinguished, each subdivided by times of extensive recession and readvance of the ice, as shown by features of the drift in this state. In the first glacial epoch, when the ice attained its greatest area, all of Minnesota except its southeast corner was deeply covered by the continental ice-sheet, and its border was several hundred miles south of this district, in Nebraska, Kansas, Missouri, and southern Illinois, Indiana and Ohio, extending somewhat beyond the Missouri river, but terminating north of the Ohio river, except in the vicinity of Cincinnati, where it reached a short distance across that river into Kentucky, as recently proved by Professor Wright. In the later very severely cold epoch, the ice-fields were of less extent, and terminated in the central part of the United States from fifty to three hundred miles within their earlier limit, covering all the basin of the Minnesota river, but not enveloping a large

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tract in the southwest corner of Minnesota and leaving uncovered a much larger area than before in the southeast part of the state. The terminal moraines which form conspicuous belts of rolling and hilly drift in Wisconsin, Minnesota, Iowa and Dakota, were accumulated at the boundaries of the ice of the last glacial epoch. Between these epochs the ice was melted away within the basins of the Minnesota and Red rivers, and probably from the entire state. The greater part of the till appears to have been deposited by the earlier ice-sheet; and during its retreat this till was overspread in some places, especially along the avenues of drainage, by beds of modified drift, or stratified gravel, sand and clay, washed from the material that had been contained in the ice and now became exposed upon its surface to the multitude of rills, rivulets and rivers that were formed by its melting.

In the principal interglacial epoch, this drift-sheet was channelled by water-courses till its valleys were apparently as numerous and deep as those of our present streams. The interglacial drainage sometimes went in a different direction from that now taken by the creeks and rivers; and the valleys then excavated in the drift, though partly refilled with till during the last glacial epoch, are still, in some instances, clearly marked by series of lakes, as in Martin county, in the south edge of Minnesota. More commonly the interglacial water-courses must have occupied nearly the same place with the valleys of the present time; and there seems to be conclusive proof that this was true of the entire valley of the Minnesota river. A long period intervened between the great glacial epochs; the earlier ice-sheet gradually retreated northward; a lake was formed in the Red river valley by the receding ice-barrier on the north; the outflow from this lake, and the drainage of the Minnesota basin itself, appear to have excavated the valley of the Minnesota river nearly as it now is; and the further recession of the ice-sheet probably even allowed the drainage of the Red river basin to take its course northward, as now, to Hudson bay, this being indicated by fossiliferous beds, containing the shells and vegetation of swamps, and trunks of trees, underlain and overlain by till, within the area that had been covered by this interglacial lake and was afterward occupied by Lake Agassiz at the close of the last glacial epoch.

Since all this, a severely cold climate again prevailed, accumulating a vast sheet of ice once more upon British America and

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the greater part of Minnesota. Beneath this glacial sheet the valley of the Minnesota river was partly refilled with till, but it evidently remained an important feature in the contour of the land surface. Perhaps it had been the pathway, along the lower part of its extent, of a subglacial river during the later epoch of ice. At the final melting of this ice-sheet, its waters, discharged in this channel, quickly removed whatever obstructing deposits of drift it had received and undermined its bluffs, giving them again the steep slopes produced by fluvial erosion. This partial excavation and sculpture anew were then immediately followed, during the retreat of the ice-sheet, by the deposition of the modified drift seventy-five to one hundred and fifty feet deep, remnants of which occur frequently as extensive terraces on the sides of this valley, from its mouth to New Ulm, and less distinctly beyond. Had not the great valley existed nearly in its present form through the last glacial epoch, it could not have become filled with this modified drift, which must belong to the era of melting of the last ice-sheet.

Modified drift, which was probably deposited during the recession of the ice of the earlier glacial epoch, was observed near New Ulm, forming a thick bed of stratified gravel and sand enclosed in the till. The section showing this is on the extension of Center street, a half mile west of New Ulm, where it rises to the top of the bluff, one hundred and eighty feet above the river, but only some one hundred feet above its old channel which lies between New Ulm and this bluff. The height here reached is the general level of the vast prairie of gently undulating till, through which the Minnesota valley is excavated. The grade cuts to a depth of about forty feet at the edge of the bluff, and thence ascends, with decreasing depth of cut, along a distance of about twenty-five rods, to the surface of the drift-sheet. This excavation exhibits two deposits of true till, separated by modified drift which is probably an interglacial formation, supplied at the time of final melting of the earlier ice-sheet and spread beyond its receding margin upon the unchannelled surface of the till that had been formed during that earlier part of the ice age. The upper bed of till, thus apparently representing the total thickness of the drift deposited here in the last glacial epoch, is sixteen to eighteen feet thick, and is an entirely unstratified yellowish gravelly clay, containing occasional rock-fragments up to six or eight inches in

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diameter, but showing only two or three of larger size, these being two to three feet in diameter. The bottom of this upper till, seen clearly exposed along a distance of about two hundred and fifty feet, is an almost exactly level line. Next below is the modified drift which is supposed to have had its origin from the melting ice-sheet of the earlier glacial epoch. Its thickness is also sixteen to eighteen feet, and consists of yellowish gravel and sand, containing pebbles up to six or eight inches in diameter, quite ferruginous in the lowest one to three feet, levelly stratified throughout, but having the horizontal layers often obliquely laminated, the dip of this lamination being to the east or northeast, toward the Minnesota river, and varying in amount from two or three to fifteen or twenty degrees. The underlying till was seen along an extent of one hundred feet, the greatest depth cut into it being about eight feet. Its upper line, separating it from the modified drift, is approximately level but undulating, with its highest points two or three feet above the lowest. This till, like the upper bed, bears no marks of stratification; and neither shows any interbedding or transition, but both are bounded by definite lines, at their junction with the intervening gravel and sand. The lower bed of till is dark bluish, excepting for about twenty feet from the face of the bluff inward, where weathering has changed it to the same yellow color that characterizes the modified drift and upper till. In other portions of the Minnesota valley, its bluffs frequently exhibit modified drift interbedded, sometimes in deposits of large extent and thickness, with the till, which makes up the principal mass of these bluffs and of the drift-sheet.

A peculiar stratification observed in several of the deposits of clay, which form part of the terraces of modified drift in the Minnesota valley in Scott and Carver counties, belonging to the time of departure of the last ice-sheet, appears to afford a measure of the rate of deposition. In Mr. Charles Rodell's excavation for brick-making at Jordan, the clay is bedded in distinct horizontal layers from three to eight inches thick, averaging six inches. These layers are dark bluish, often finely laminated, changing above and below to a nearly black, more unctuous and finer clay, which forms the partings between them. These divisions are clearly seen through the whole extent of this excavation, which reaches twenty-five feet below the top of the clay and is four rods long. The height of its top is estimated to be sixty-five feet

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above the river. The excavation of Nye & Co., at Carver, where the exposure is four rods long and fifteen feet high, with about the same elevation above the Minnesota river as the foregoing, exhibits the same stratification, except that here the layers all have a nearly uniform thickness of three inches. There is a tendency to split at the darker partings, which are seen to extend continuously, never passing one into another, and preserving a very constant width of three inches apart, through the whole of the section exposed. They are from an eighth to three quarters of an inch thick, gradually merging above and below into the less dark clay that makes up the principal mass of these layers. The bedding is nearly level, but dips one to two degrees away at each side. In this depth of fifteen feet there are thus about sixty layers, all closely alike. The alternating conditions which produced them were evidently repeated sixty times in uninterrupted succession. The only explanation of this which seems possible is that these divisions mark so many years occupied by the deposition of this clay. Layers nearly like those in the clay at Carver and Jordan are also seen in other clay-beds in this valley and in that of the Mississippi in this state. The principal mass of each layer is regarded as the deposition during the warm portion of a year, and the very dark partings as the sediment during winter when the glacial melting was less and the water consequently less turbid.

At Chaska, situated in the Minnesota valley two miles below Carver, the clay used for brick-making is modified drift of interglacial age. It varies from twenty to forty feet in thickness, being underlain by sand and covered by till from two to six feet thick, holding boulders of all sizes up to five or six feet in diameter, many of which are planed and striated. This till forms the surface, thirty to thirty-five feet above the river. The only fossils found here were fresh-water clam shells which occurred in considerable numbers upon a space four rods in diameter near the middle of Gregg & Griswold's excavation, lying in the upper foot of the clay, just beneath the till. This interglacial clay, overspread by till, testifies that an ice-sheet covered this region after the Minnesota valley had been eroded nearly as it now is.

Another observation which seems to give the same testimony, and to show that the modified drift forming high terraces and plains in this valley was deposited during the recession of the ice-

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sheet, is presented in the notably uneven surface of the broad part of the terrace of this valley drift in Carver county between Carver and Beven's creeks. On this tract, composed, below the soil, of stratified gravel and sand, extending about two miles in width and elevated one hundred and twenty-five feet above the river, are frequent depressions from ten to thirty rods in diameter and fifteen to forty feet in depth below the general level, often enclosed without outlet, and some of them containing lakelets and sloughs. Such hollows have not been seen elsewhere in our exploration of these terraces along the Minnesota valley, which instead have generally a smoothly level contour. Their origin must apparently be referred to sedimentation, while masses of ice occupied the places of these bowl-like depressions. Elsewhere the absence of such irregularities in the surface of the valley drift, as also the very rare occurrence of boulders in it, and the fact that no portion of it, excepting that just mentioned at Chaska, is known to be interglacial by having become covered with till, together show that the deposition of these beds of modified drift took place outside the limits of the retreating ice-sheet. The valley appears to have remained from excavation in an interglacial epoch, and to have become rapidly filled with sediments as soon as the ice by which it had been enveloped was melted away.

Alluvial beds fill the Minnesota valley at Belle Plaine, as shown by the section of the salt-well, to a depth about 150 feet below the present river at its stage of low water. This well, situated on the bottomland at nearly the same height with the depot, or approximately 30 feet above the river and 725 above the sea, is reported by Prof. Alexander Winchell to have passed through the following succession of deposits before reaching the bed-rock; soil and gravel, 9 feet; clay and gravel, 9 feet; sand and gravel, 18 feet; quicksand, 54 feet, having its base 90 feet below the surface; coarse sand, 1 foot; clay, 6 feet, in which was found, two feet from its top, a piece of grapevine with bark; sand, 38 feet, varying from quicksand to coarse sand, in which at 114 feet, inflowing water, under pressure from the bottom, filled the pipe twelve feet with sand, and a second time, at 125 feet, filled it five feet; then, gravel, quicksand, and coarse sand, 45 feet, having its base 180 feet below the surface, yielding water at 144 feet, which filled the pipe with sand ten feet, and containing another piece of grapevine at 168 feet; next, from 180 to 200 feet, blue clay, 7 feet, and rock-

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fragments, 13 feet, probably both boulder-clay or till; and, lastly, gravel, 2 feet; the whole depth of alluvium and drift being thus 202 feet, extending about 170 feet below the river.

At the railroad bridge which crosses the Minnesota river close to its mouth, borings were made to a depth of 60 feet below the river-level without reaching the bed-rock. In the deep well at Mankato, drift was found to extend 65 feet below the river.

A summary of the glacial history of the Minnesota valley, as recorded in its physical and geological features here described, is nearly as follows: This channel, excavated in the Lower Magnesian or Calciferous formations far below the bottom of the present valley, appears to have been eroded by a river during the later Palæozoic and earlier Mesozoic ages, before the Cretaceous subsidence which carried much of this state, with a large area farther west, beneath the sea. In the first principal epoch of glaciation, when the ice covered its greatest area, a thick drift-sheet, mostly unmodified, was spread over all this region, probably covering most of this preglacial valley with an unbroken, moderately undulating expanse of till. During the ensuing interglacial epoch, the drainage of this area cut a channel, which, because of the natural slopes of the basin determined by preglacial erosion, coincides along much of its lower part, where it crosses the nearly horizontal Palæozoic formations, with the old valley eroded in these strata long before the ice age. The preglacial, and probably also the interglacial river lay far below the present stream. The till of the later glacial epoch appears to have only partially blocked up this river-course along the greater part of its extent, and portions which may have been obstructed were soon channelled anew, and this valley from its mouth to New Ulm or beyond was filled with modified drift, to the height of its present terraces of this formation, during the recession of the last ice-sheet. After the departure of the ice from the Minnesota basin, this avenue of drainage continued through a long time to be the outlet of Lake Agassiz, whence it received an immense volume of water, supplied from the melting ice-fields of northwestern Minnesota and of a vast region reaching far to the north and northwest over the basin of Lake Winnipeg and the Saskatchewan river. As long as streams poured into this valley directly from the melting ice-sheet, its modified drift, gathered from the ice in which it had been held, continued to increase in depth; but when the great glacier had retreated

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beyond the limits of the basin of the Minnesota river, the water discharged here from Lake Agassiz brought no modified drift, and was consequently a most powerful eroding agent. By this mighty river the valley drift, so recently deposited, was mostly swept away, and the channel was excavated to a depth lower than the present river and perhaps quite to the bottom of the gravel and sand in this valley at Belle Plaine, which is 150 feet below the river there and about 140 feet below low water in the Mississippi at Saint Paul. Since the ice-barrier which had caused Lake Agassiz disappeared and that lake was drained northeastward to Hudson bay, the Minnesota valley and that of the Mississippi below, carrying only a small fraction of their former volume of water, have become considerably filled by the alluvial gravel, sand, clay and silt, which have been brought in by tributaries, being spread for the most part somewhat evenly along these valleys by their floods.

The changes produced by this postglacial sedimentation have been pointed out and ably discussed by Gen. G. K. Warren, who thus added much to our knowledge of the geological history of the Minnesota and Mississippi rivers. Lakes Traverse, Big Stone and Lac qui Parle occupy hollows in the outlet of Lake Agassiz due to inequalities of these recent deposits. At the mouth of the Minnesota river, the Mississippi has brought more sediment than its branch, which is thus dammed for a distance of thirty miles to Little Rapids, with a depth of twenty to twenty-five feet at low water. The current of this part of the Minnesota through the dry season is very sluggish or imperceptible, and its surface often becomes considerably covered with the green scum of cryptogamous vegetation characteristic of pools and lakes. The channel here is from fifteen to twenty-five rods wide, with no lake-like expansions; but lakes from one to four or five miles long, and from a quarter to a half mile wide, lie near the river and parallel with it at each side, upon the bottomland. Lake Pepin, having a depth of about sixty feet, according to General Warren, lies in the continuation of this valley which was deeply channelled by the outflow from Lake Agassiz, because it has become unequally filled below the foot of this lake by the deposition of alluvium from the Chippewa river. Two of the tributaries of the Mississippi from the east were similar outlets of floods sup-

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plied by glacial melting after they had become free from their modified drift by flowing through a lake. Lake Superior, held by an ice-barrier on the northeast at a level about 500 feet above its present height, overflowed at the head of the Bois Brulé river, by Upper St. Croix lake and the St. Croix river. The Mississippi valley at the mouth of this river, as in the case of the Minnesota river, has become more filled by postglacial deposits than its tributary, which is thus held as back-water twenty miles, to the head of Lake St. Croix, which is twenty-five feet deep. Lake Michigan, till the receding ice-sheet was melted from its present outlet at the north, similarly discharged southward by the Illinois river, which, like the foregoing, is obstructed at its mouth by the alluvium of the Mississippi. At low water the greater part of its length is dammed, and has a very slight and often imperceptible current through the two hundred miles from La Salle by Lake Peoria to its mouth. Major Long remarked: "This part of the river may, with much propriety, be denominated an extended pool of stagnant water." All these results of recent fluvial action show that the drainage from the final melting of the ice-sheet excavated these valleys to depths much lower than they have now, and make it very probable that the deposits penetrated by the first 180 feet of the Belle Plaine salt-well are wholly postglacial.

The exposures of rock over which the Minnesota river flows at Little Rapids, ten miles below Belle Plaine, do not forbid this conclusion, for the topography of the valley in that vicinity indicates that a much deeper channel than that now occupied by the river may have existed there since the ice age, passing a mile and a half east, and a mile northeast of the Little Rapids. This course of the river along which it is believed to have cut a channel in the easily eroded Jordan sandstone to a depth equal to that of the fluvial deposits penetrated by the well at Belle Plaine, extends northeastward diagonally across section five, Sand Creek; then northerly through the west part of section thirty-three, Louisville; and thence northwesterly through section twenty-nine. It thus leaves the present river a mile south of these rapids, and returns again to it about a mile south of Carver, after passing east and north of the island-like sandstone outcrops of section thirty-two, Louisville. The recent accumulation of sediments that fill this

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avenue to a height slightly above the Little Rapids, has turned the river that way, so that it has abandoned its former course and now flows over ledges of sandstone.

Four features of the glacial drift in Minnesota seem to me very impressive and even grand. These are, first, the great thickness of the drift and its extent over large areas where it conceals and deeply covers all the surface of the older bed-rocks; second, the terminal moraines; third, Lake Agassiz; and, fourth, the outlet from this lake, which has been the theme of the present paper. Professors J. D. Dana and J. W. Spencer, to the latter of whom we are indebted for valuable work in the Quaternary geology of the region of lakes Erie and Ontario, have expressed the desire, in which I heartily join, to recognize in the nomenclature of American geology the work of the engineer who in the years 1866 to 1869 made a survey of the Minnesota valley from Big Stone lake to its mouth, and from whose observations and writings, already several times referred to, this essay has received most important contributions and suggestions. Therefore it seems fitting to propose here for the ancient river which flowed in the Ice Age where lakes Traverse and Big Stone and the Minnesota river now are, this name, the *River Warren*, in honor and *in memoriam* of Gen. G. K. Warren, the author of the first adequate description of this valley.

Changes in the Currents of the Ice of the Last Glacial Epoch in Eastern Minnesota.

By Warren Upham of Minneapolis, Minn.

This paper is based upon my observations as assistant on the geological and natural history survey of Minnesota, under the direction of Prof. N. H. Winchell, the state geologist. It has been previously read, May 8, 1883, before the Minnesota Academy of Natural Sciences.

When the ice of the last glacial epoch attained its maximum extent, it appears that the ice-current moving southwestward from lake Superior across the northeast part of Minnesota, spreading a reddish till containing boulders and pebbles peculiar to the region from which it came, had its limit at a line reaching from

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lake St. Croix southwesterly across the Mississippi and through the north part of Dakota county, thence bending to a northwest direction and continuing by lake Minnetonka and through Wright and Stearns counties. At the same time another portion of the ice-sheet was pushed from the region of lake Winnipeg and the Red river valley toward the south and southeast, meeting and opposing the ice-current from lake Superior along a line from Stearns county southeast by lake Minnetonka to Crystal lake in Dakota county, beyond which its eastern limit farther south was at the outer or eastern belt of the terminal moraine as traced in Rice, Steele and Freeborn counties and onward to central Iowa. The angle formed by the margins of these portions of the ice-sheet, moving respectively from the northeast and the northwest, was at Crystal lake and Buck hill in Burnsville, Dakota county; from which point northwesterly, along an extent of a hundred miles or more, these ice-currents were pushed obliquely against each other.

At a later date in this last glacial epoch, after the ice-border had for the most part retreated considerably from its farthest limits, it is known to have halted in its recession (and it even probably reädvanced), forming a second belt of morainic accumulations usually from five to fifteen miles back or inside from its former line of drift-hills. This second morainic belt is well exhibited from Rice county south to central Iowa, and at the west side of this lobe of the ice-sheet upon the Coteau des Prairies. At this time the ice-sheet that was pushed southwestward from lake Superior and northern Wisconsin probably terminated only a few miles back from its earlier limit east of lake St. Croix and in Washington, Dakota and Ramsey counties, and in the east edge of Hennepin county at Minneapolis. But farther northward the presence of bluish till, weathered next to the surface to a yellowish color, containing boulders and pebbles of limestone and of Cretaceous shale and other material brought from the northwest, overlying the red till with rock-fragments from the region of lake Superior, proves that the ice-current from the northwest was stronger and extended farther, the ice on this side of the great western lobe of the ice-sheet being therefore even deeper, at least in comparison with the thickness of the eastern ice flowing southwestward from lake Superior, than in the former part of this glacial epoch when these ice-fields covered their greatest area.

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In that earlier part of this epoch the ice-currents from the northwest and northeast had met along a line drawn from Crystal lake and lake Minnetonka northwestward; but now, when elsewhere the border of the ice-fields had somewhat retreated and formed the second and inner terminal moraine, the ice flowing from the west extended eastward across Wright and Hennepin, Sherburne and Anoka counties, to the St. Croix river at the east side of Chisago county, and into the adjoining edge of Wisconsin, pushing back the ice-current that came from lake Superior, and covering the red till brought by that ice with the characteristic blue till brought by the ice from the northwest and west.

The cause of this changed course of the line at which the currents of the west and east portions of the ice-sheet met is to be found in the changed meteorological conditions of this time. During the increased ice-melting attendant upon the recession of the ice-fields from the outer to the inner terminal moraine, the prevailing westerly winds sweeping over the western side of the ice-sheet upon the Coteau des Prairies and eastward became more laden with moisture than in the earlier part of this epoch when there was comparatively little melting upon the surface of the ice; and the increased temperature enabled these winds to carry their moisture farther than when the ice had its greatest extent. Then the precipitation of rain and snow took place more upon the western side of the ice; but at this later time the precipitation, by reason of the causes here mentioned, probably became much greater than before upon the east part of the lobe of the ice-sheet that extended southeastward from the Red river valley to central Iowa. Before this, lake Minnetonka and central Wright county had been the limit where this ice-flow was stopped by the opposing ice-current from lake Superior; but now, because of the relatively and perhaps absolutely greater thickness of this part of the ice flowing from the northwest, due as shown to climatic changes, its current pushed back that opposed to it on the east, covering the red till brought by that ice with blue till containing plentiful limestone boulders and other material from the west and northwest.

The line where these ice-fields moving from the west and from the northeast now met, lies in the south edge of Mille Lacs, Kanabec and Pine counties, and even beyond the St. Croix river at the east side of Chisago county, fully seventy-five miles east of the line where these currents formerly met; but it scarcely reaches into Washington

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and Ramsey counties, which remained covered with ice that came from the northeast. This persistence of the ice-flow from the northeast near the margin of the ice-covered area, and also in Stearns and Morrison counties, north of the changed portion of the line of confluence of these ice-fields, proved by the character of the drift upon these counties, seems yet quite consistent with this explanation, by meteorological causes, of the change in source of the ice covering the intervening district, from Wright and Hennepin counties eastward to the St. Croix river and the edge of Wisconsin.

Details of sections in the drift showing the blue (or next to the surface yellow) till overlying the red till in this district, and more complete discussion of the glacial period, its ice-sheets, and their various drift deposits in this state, have been partially presented in the fifth, sixth, eighth and ninth annual reports of the geological and natural history survey of Minnesota, and will be fully exhibited in the final reports of this survey.

THE KAME RIVERS OF MAINE. By GEORGE H. STONE of Colorado Springs, Colorado.

[ABSTRACT.]

NEARLY all glacialists now agree that most of the kame gravels of the drift region were deposited by glacial streams. There remains a difference of opinion as to whether these streams were sub- or super-glacial; but for the sake of brevity no reference is here made to the literature of the subject. In my studies of the drift in Maine, I think I have found evidence of both kinds of streams.

During an exploration of Maine which continued for about five years. I had opportunity to examine many thousands of sections of the till throughout the greater part of the state. I have never found stratified or even water-classified material enclosed in this formation, except within a few miles of the coast, or so near to the surface as to leave the interpretation in doubt. Hundreds of miles of the beds of the inland streams have been examined by me, and if water-washed material exists in the till in the interior of

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the state, it must be rare. But near the coast, as for instance at Portland, one may see small masses of sand, nearly or quite surrounded by the unmodified till, the clayey till being in such contrast with the clean, silicious sand, that the difference in composition and mode of formation is seen at once unmistakably. These masses of sand and gravel in the till are often not now stratified, but all have been plainly classified by running water and not afterward so mixed with the clayey till as to lose their identity. If a ribbon or pocket of sand, not an inch in thickness, can be thus plainly distinguished from the amorphous till, surely the absence of such deposits from the great interior region is a strong indication that the conditions were there different from what they were near the coast. A somewhat positive indication of what this difference was is found in the existence of pot-holes near the coast in situations where no ordinary surface streams can ever have flowed. These pot-holes must be regarded as the work of glacial streams. In the interior no pot-holes have been found except in the valleys of ordinary streams.

Before interpreting these facts, let us consider the causes of the melting of a glacier. The white color and porous structure of the surface ice of a glacier show where most of the melting takes place. This superficial water flows along the surface, or it may wear and melt a deeper and deeper channel until it disappears down some crevasse and becomes a sub-glacial stream. Modern glaciers show a difference in their glacial streams. Near the lower extremity almost all the water is sub-glacial. The ice is so broken by crevasses that the melting waters almost immediately plunge to the bottom. But as we go backward toward the line of perpetual snow, where the ice is thicker and the melting less rapid we find streams upon the surface, the length depending upon the extent of ice free from crevasses. How far do these conditions represent those of the great glacier of eastern North America?

Without some limitation, it is doubtful if the analogies of any modern glaciers, even of Greenland glaciers, can be admitted as valid in the case of the great ice-sheet. The Greenland glaciers do not wholly overlap large areas like the great ice-sheet, but here and there peaks emerge from them which form crevasses and assist the waters to pass beneath the glacier. However, waiving these objections, and admitting for the time that the sub-glacial streams of our great glacier were as well developed as those of the Green

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land glaciers of to-day, at the same time we must admit that the surface streams were as well developed. Now the superficial streams of the Greenland glaciers are often of considerable size; therefore we must believe that large surface streams existed upon the ice-sheet at a proper distance north from its terminus. It should be noted that this is the condition of the Greenland glaciers during a time of comparative stability, or, quite probably, of their slow increase in extent and depth.

But what would happen during the final melting of the ice-sheet? All substantially admit that this melting was very rapid and that it took place upon a great width simultaneously, although the ice, being thicker toward the north and in a colder mean climate, would not become melted so quickly as farther south. As I have elsewhere pointed out, no southward motion would be possible over a large part of Maine, after the thickness of the ice was reduced to about five hundred feet, on account of transverse ranges of hills. Nowhere except in the White Mountains have I found traces of glaciers following local valleys. Apparently, almost all flow ceased after the melting had so far proceeded that the hills appeared above the ice. Under these conditions no new lines of crevasses would open in the thinning ice-fields, even after the hills began to emerge. The melting took place so rapidly, and the motion of the ice ceased so completely that there would be no time for the formation of sub-glacial channels. On the contrary, the exigency of glacial melting, supplemented by the summer precipitation, would cause a great extension of the superficial streams.

The presence of glacial pot-holes in Maine near the coast, and the masses of water-washed matter in the till, show that the glacial streams there reached the ground. The most of the area of sub-glacial streams must have lain south of the present coast-line. The glacier reached out into the Gulf of Maine, and off its front were probably numerous boiling springs of fresh water, like those at the submerged extremities of the Greenland glaciers. The ice over the great interior region of the state was for the most part drained by superficial streams. When the melting had so far proceeded that the bottoms of these streams reached the moraine-stuff which was scattered through the lower part of the ice, then the kame-gravel began to gather on the bottoms of their channels. Several of these kame-rivers were a hundred or more miles long, as

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is shown by my map of the kames of Maine, published in the *Proceedings* of this Association for 1880. Most of the kame-rivers have been described by me as probably flowing in channels formed superficially on the ice, and that is my opinion still. Near the coast these channels may originally have been of sub-glacial origin. Northward there was a region where the drainage of the glacier was chiefly by surface channels which finally found their way beneath the ice at the southern limit of this region. The final melting of the great glacier took place largely at the surface, and this melting together with the large rainfall, caused great bodies of water to collect on the ice, finally wearing and melting deep channels. In some cases these streams probably followed previously formed sub-glacial channels for a part of their course, widening them until the ice-arch collapsed by its own weight.

We may grant as great a development of sub-glacial streams in the ice-sheet as anyone can reasonably consider is demanded by the Greenland glaciers, and yet I cannot see how this furnishes any adequate representation of the conditions during the final melting, when there was such a vast accession of surface water, and also when the ice-flow had so far ceased as to make it certain that few if any additional crevasses would be formed. Under these circumstances there could be no time to extend the previously formed sub-glacial channels far, and the sudden floods would find their way over the lowest parts of the ice just as they would over the ground. A great and sudden northward extension of the superficial streams would be the result.

The above reasoning applies to Maine only, and other regions are to be studied by themselves. Part of the reasoning will probably apply to most or all of the glaciated area of North America, but with many differences in the details. Indeed, as glacialists come to better appreciate the great variety of work which the ice-sheet performed, the less likely shall we be to fight for the privilege of imposing the local conditions, with which we are most familiar, upon the whole drift-covered area.

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On Glacial Canons. By W. J. McGee of Washington, D. C

[ABSTRACT.]

Glacial cañons are characterized by several peculiar features, of which the greater number suggest that glaciers are most effective engines of erosion, while others indicate that glacial erosion is inconsiderable. Neither the source of this apparent discrepance nor the full significance of the characteristic features of such cañons can be well determined empirically; but on approaching the subject from the analytical side, it appears, from detailed consideration of the several agencies involved in the development of the leading features, that the effect of *temporary* occupancy of a typical water-cut cañon by glacier ice will be to (1) increase its width, (2) change the original V to a U cross profile, (3) cut off the terminal portions of tributary cañons and thus relatively elevate their embouchures, (4) intensify certain irregularities of gradient in the cañon-bottom, (5) excavate rock-basins, (6) develop cirques, and, in general, transform such cañon into an equally typical glacial cañon.

Clay Pebbles from Princeton, Minn. By N. H. Winchell of Minneapolis, Minn.

[ABSTRACT.]

This paper was accompanied by an exhibition of specimens. The pebbles were of various shapes and sizes, most of them somewhat cylindrical. Outside, they are coated with fine sand and gravel; inside, they consist wholly of a fine sedimentary clay, such as is deposited by standing water, and contain no interior pebbles. Professor Winchell had compared these with pebbles of till found in deposits of gravel and sand and with various others, without finding anything exactly similar.

Professor Newberry examined the pebbles, and admitted that they were not exactly like any that he had seen, but he thought they bore a general resemblance to pebbles found throughout the range of the Carboniferous strata wherever there is a bed of sandstone capped by clay. Professor Claypole claimed to have seen similar specimens in the drift of Pennsylvania.

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THE PRE-CAMBRIAN ROCKS OF THE ALPS. By T. STERRY HUNT of Montreal, Canada.

[ABSTRACT.]

THE writer began by reviewing the history of Alpine geology, and noticed first that speculative period when the crystalline rocks of the Alps, including gneisses, hornblendic and micaceous schists, euphotides, serpentines, etc., were looked upon as altered sedimentary strata of carboniferous or more recent times. He then traced the steps by which these views have been discarded, and more and more of the rocks shown to belong to eozoic or pre-Cambrian ages. In this connection the labors of von Hauer, Gerlach, Heim, Favre, Renevier, Lory, Gastaldi and others were analyzed, and reference was made to the great progress since the writer, in 1872, published a review of Favre's work on the geology of the Alps.

The sections by Neri, Gerlach and Gastaldi in the Western, and those of von Hauer in the Eastern Alps were described, and it was shown that all these agree in establishing in the crystalline rocks four great divisions, in ascending order: 1st. The older granitoid gneiss with crystalline limestones, graphite, etc., referred by Gastaldi to the Laurentian. 2d. The so-called *pietre verdi*, or greenstone group, consisting chiefly of dioritic, chloritic, steatitic and epidotic rocks, with euphotides and serpentines, including also talcose gneisses, limestones and dolomites, and regarded by Gastaldi as Huronian. 3d. The so-called recent gneisses of von Hauer and Gastaldi, interstratified with and passing into granulites and micaceous and hornblendic schists, also with serpentines and crystalline limestones. 4th. The series of argillites and soft glossy schists with quartzites and detrital sandstones, including also beds of serpentine with talc, gypsum, karstenite, dolomite, and much crystalline limestone. This fourth series, well seen at the Mount Cenis tunnel, is still claimed by Lory and some others as altered trias, but the present writer's view, put forth in 1872, that it is, like the preceding groups, of eozoic age, was subsequently accepted by Favre, and by Gastaldi, and is now established by many observations. To this horizon belong the crystalline limestones of the Apuan Alps, including the marbles of Carrara.

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The writer next recalls the fact that he, in 1870, insisted upon the existence of a younger series of gneisses in North America, alike in the Atlantic states, in Ontario, and to the northwest of Lake Superior. These, in his address before the American Association for the Advancement of Science in 1871, he farther described under the name of the White Mountain series, and subsequently, in the same year, called them Montalban. These rocks were then declared to be younger than the Huronian, and to overlie it; though, in the absence of this latter, it was pointed out that in Ontario and in Newfoundland the Montalban reposes unconformably upon the Laurentian. When these newer gneisses and mica-schists were first described, in 1870, there was included with them an overlying group of argillites, quartzites and crystalline limestones, and for the whole the name of Terranovan was suggested provisionally; but in defining in the following year the White Mountain series, this upper group was omitted, and was subsequently referred to the Taconian series,— the Lower Taconic of Emmons, and the so-called altered Primal and Auroral of H. D. Rogers, in Eastern Pennsylvania.

The writer next described his own observations in the Alps and the Apennines in 1881. He affirms the correctness of Gastaldi in referring the groups one and two to Laurentian and Huronian, finds the third, or the younger gneiss and mica-schist group of the Alps, indistinguishable from the Montalban, and regards the fourth as the representative of the American Taconian. It was maintained by Gastaldi that these pre-Cambrian groups of the Alps underlie directly the newer rocks of northern and central Italy, forming the skeleton of the Apennines, reappearing in Calabria, and moreover protruding in various localities in Liguria, Tuscany and elsewhere. The serpentines, euphotides and other resisting rocks, thus exposed, have been regarded as eruptive masses of triassic and eocene time. The writer, however, holds, with Gastaldi, that they are indigenous rocks of pre-Cambrian age exposed by geological accidents.

The uncrystalline rocks of the mainland of Italy are chiefly cenozoic or mesozoic, and the only paleozoic strata known are carboniferous, the organic forms of the limestones of Chaberton having been shown to be triassic. Triassic, liassic, cretaceous, eocene and miocene strata are found in different localities resting on the various pre-Cambrian groups. In the island of Sardinia, however,

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all these are overlaid by a great body of uncrystalline lower palæozoic rocks, in which the late studies of Bornemann and Meneghini have made known the existence of a Lower-Cambrian fauna, including Paradoxides, Conocephalites and Archeocyathus; succeeded by an abundant fauna of Upper Cambrian or Ordovician age. In the island of Elba, according to Lotti, rocks containing the latter fauna repose directly upon the crystalline schists with serpentines.

The existence of the younger or Montalban gneiss in Sweden, and in the Harz and the Erzgebirge was noticed, and to it were referred the Hercynian gneisses and mica-schists of Gümbel. The presence both in Sweden and in Saxony of conglomerates, as described by Hummel, and by Sauer, wherein pebbles of the older gneiss are enclosed in beds of the younger series, was discussed, and the direct unconformable superposition of the latter upon the older gneiss, in the absence of the Huronian, was considered; evidences of the same relations being adduced from the Alps. The gneisses of the St. Gothard, as seen on the Italian slope, were also referred to the newer series, and the important studies of Stapff, in this connection, were discussed. It was declared that the views put forth by the author in 1870–71 on the relations and succession of the crystalline stratified rocks in North America, and then extended by him to Europe, have been fully confirmed by the labors of a great many European geologists, as already shown. Those of Hicks, Hughes, Bonney, Callaway, Lapworth and others in the pre-Cambrian rocks of the British islands were cited in support of these conclusions. It was said that whatever may have been the conditions under which these vast series of crystalline stratified rocks were deposited, there is evidence in the similarity of their mineralogical and geognostical relations, of a remarkable uniformity over widely separated regions of the earth's surface, as well as of long intervals of time, marked by great foldings and disturbance, and by vast and wide spread erosion of the successive series of rocks.

In conclusion, the writer took occasion to call attention to the important labors of the present school of Italian geologists, and their great zeal, skill and disinterested service, as shown in the memoirs of the R. Accademia dei Lincei, and in the work of the Royal Geological Commission, including the special studies, maps

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and memoirs prepared by it for the International Geological Congress of Bologna in 1881. The new Geological Society of Italy, founded at the same date, gives promise of a brilliant future, and has already published many important memoirs.

The Serpentine of Staten Island, New York. By T. Sterry Hunt of Montreal, Canada.

[ABSTRACT.]

The serpentine of Staten Island appears as a north-and-south range of bold hills, rising out of a plain of mesozoic rocks. On the west side are triassic sandstones like those of the adjacent mainland, including a belt of intrusive diabase, and on the east the overlying and nearly horizontal cretaceous marls, which are traced south and west into New Jersey. The only rocks besides those here mentioned, seen on the island, are small areas of coarse-grained granite, having the characters of a veinstone or endogenous mass, and others of an actinolite-rock, both exposed among the sands on the northeast shore of the island.

Mather, who described this locality forty years since, looked upon the serpentine as an eruptive rock, like the parallel belt of diabase seen to the west of it, but Dr. N. L. Britton, of the New York School of Mines, who in 1880 published in the Proceedings of the New York Academy of Sciences a description and a geological map of the island, regarded the serpentine as similar to that which is stratified as a contemporaneous member in the ancient gneissic series of Manhattan island, and appears also at Hoboken; a view which is doubtless correct.

The appearance of isolated hills of serpentine rising among newer rocks is common in other regions, and is by the writer attributed to the fact that this insoluble magnesian silicate resists, to a great degree, the action of subaërial decay, which converts gneisses and other feldspathic rocks into a clay, that is easily removed; leaving the beds and lenticular masses of interbedded serpentine in relief. In many parts of Italy, where ridges or belts of serpentine protrude in the midst of tertiary strata, they have been des-

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cribed as eruptive masses. The question of their geognostical relations is there often complicated by the fact that subsequent movements of the earth's crust have involved alike the underlying serpentines and the newer strata, and have given rise to faults and inversions, by which the younger rocks, overturned, are made to dip towards and even beneath the older. This condition of things the writer illustrated by references to localities of serpentine lately examined by him in Liguria, and in Tuscany, where the true geognostical relations of these rocks was first indicated by Gastaldi. The structure above described was further explained by reference to the similar inversions of strata to be seen along the western base of the Atlantic belt, from the Highlands of the Hudson southward, along the Appalachian valley.[1]

The writer stated that although serpentine, under subaërial influences, decays much less rapidly than the harder gneisses, it does not entirely escape this process. He showed that this rock on Staten Island is covered in parts with a decayed layer, including portions of limonite, separated by a process of segregation in pre-glacial times, since in the subsequent erosion it has been removed from many parts of the serpentine-belt. The details of this decomposition of the serpentine, and of its relation to glacial erosion have been discussed by the writer in an essay on rock-decay, to appear in the *American Journal of Science* for September, 1883. He acknowledged, in conclusion, his obligations to Dr. Britton for his careful descriptions, and for his personal guidance on Staten Island.

On the Genesis and Classification of Mineral Veins. By J. S. Newberry of New York, N. Y.

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[1] The history of the serpentine of Staten Island and of other American localities, as well as those of Italy, will be found treated at length in the author's paper entitled The Geological History of Serpentines, with notes on pre-Cambrian Rocks; which appears in the *Transactions of the Royal Society of Canada* for 1882–1883. Vol. I, pp. 165 – 215.

On the geological map of the state of New York, published "by legislative authority" in 1842, the serpentine of Staten Island, the diabase adjacent, and that of the Palisade range, are alike colored as "Trap Rocks."

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On The Hamilton Sandstone of Middle Pennsylvania. By E. W. Claypole of Buchtel College, Akron, O.

[ABSTRACT.]

The Hamilton sandstone is a peculiar local development in the middle of a group composed generally, in eastern North America, of soft shales. The middle of the Hamilton group begins to grow sandy near the Schuylkill River, the proportion of sand increasing to the southwest, to the Susquehanna, when it consists of a heavy bed of sandstone 800 feet thick and very solid. Hence its sand continues but grows somewhat less in quantity to its last appearance at twenty miles farther to the southwest. The breadth of this belt of sand is not great. It rapidly shales off to the northeast and is scarcely recognizable thirty miles from the Blue Mountains its most southerly exposure.

The Hamilton sandstone may consequently be regarded as an elongate fan-shaped mass of sand intercalated in the Hamilton shales and thinning off in all directions from a single point. So far as it is possible to determine it, this point is on or near the southeastern point of Perry County.

There is no doubt that this sandstone once extended farther to the southward but the enormous erosion that has taken place has removed all traces of it south of the Blue Mountains; yet what remains is enough to enable us to form an idea of its extent when deposited.

The question naturally arises, what was the source of this vast mass of sand deposited in this place and in this form?

The distribution does not resemble that of the other sandstones in the county. It is comparatively small in extent and far from uniform in thickness and consistency. The obvious cause that first suggests itself is that we have the deposit of a large river of that day which brought down from a continent a quantity of sediment whereof the coarser part was dropped close or near to shore and the finer carried off to a greater distance. In that view of the case the river must have been near the site of the present Susquehanna but must have flowed in an opposite direction. And the source of the material must have been a large tract of land lying to the south or southeast of Perry County, the mouth of the ancient stream being near the present site of Harrisburgh. Such a tract of land may have consisted

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of the area of primal sandstone in York and Lancaster counties if at that day exposed, which it would be difficult to disprove, or else the great area of gneiss ranging southwest from Philadelphia. In either case the supply of sand would be ample.

Several facts may be quoted in support of the opinion above advanced. In the southeast of Perry County the geological column is incomplete as compared with other parts. There the Hamilton sandstone lies on the top of the Onondaga red shale, all the intermediate rocks being absent, namely, the following:

Hamilton lower shale . . .	400 feet.
Marcellus black shale . . .	100 "
" iron ore . . .	100 "
" limestone . . .	
" shale . . .	
" lower ore . . .	
Oriskany sandstone	40 "
Limestone shale	200 "
Limestone	100 "
	940

with part of the upper beds of the Red Shale. It is a fair inference that the part of the county over which these beds do not occur was dry land during the latter part of the interval expressed by their absence, and that during all this latter part erosion of the beds deposited during the former part must have taken place. Supposing that some geographical change toward the close of the dry land period had the effect of throwing the drainage over this district by diverting or developing the river above suggested, we have a sufficient cause for the production and deposition, of the sand. Subsidence is the usual accompaniment of deposition and as the accumulation of sand and silt increased the region subsided extending the area covered until finally the water became again deep, the deposit of sand ceased, and was succeeded by a deposit now forming the Hamilton upper shale.

It is not necessary to assume any elevation of the land as a cause of the dry ground spoken of above in the southeast of Perry County, etc. Some irregularity in the subsidence which all now admit accompanied the enormous Appalachian deposit may have drawn the

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water down leaving certain districts of which this was a portion above sea level for a time.

This part of Perry County is the most ancient dry land which geology has yet indicated in the district. All else to the earliest accessible times was open sea.

THE EQUIVALENT OF THE NEW YORK WATER-LIME GROUP DEVELOPED IN IOWA. By A. S. TIFFANY of Davenport, Iowa.

[ABSTRACT.]

THE horizon of the even-bedded magnesian buff limestone of the Le Claire and Anamosa quarries has been a question of uncertainty and dispute and for more than twenty years organic remains have not been found. Prof. C. A. White, Vol. I, p. 182, says:

> In the former report of the Geology of Iowa, two distinct formations of the Niagara Group were recognized and described, namely, the Niagara limestones and the Le Claire limestones, the latter resting upon the former. Subsequent stratigraphical and palæontological examinations fail to show any good reasons for referring any of the Upper Silurian rocks of Iowa to any other than the epoch of the Niagara limestones; therefore they are all regarded as belonging to that formation alone. Messrs. Worthen and Meek, of the State Geological Survey of Illinois, long since came to the same conclusion, and in the recently published State reports, they reaffirm it.

Organic remains not having been discovered by these several geologists they were left with only lithology and superposition to determine the horizon. The superposition cannot be studied successfully at Le Claire.

There are, however, many places in Scott, Cedar, Clinton, and Jones counties where the superposition shows that it is superior to the Niagara. The Niagara formation in Iowa had been land, and consequently subject to denuding agencies, for a long period before the formation under consideration had begun to be deposited. The Niagara deposit had been deeply scored and grooved by the channels of drainage.

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During this period many hundreds of feet of Upper Silurian rocks were deposited in the state of New York. When subsidence had brought the Niagara below the ocean again, the deposition of the rocks under consideration took place, filling the interstices in the eroded Niagara group. Being thus protected by the older and harder Niagara rocks, they have escaped subsequent erosion, while the higher portions, which once connected these numerous outliers, have been swept away.

Prof. Hall, in the New York report of the Geology of the Fourth District, p. 142, Fig. 57, in describing the Water-lime group, figures some acicular crystals of sulphate of baryta which are numerous in this group of rocks at Anamosa.

The writer had been diligently seeking for organic remains for more than twelve years, and was at last rewarded in February, 1882, by finding great numbers of fossils in the stone from the base of the Anamosa quarries, all of which are of the type of the Water-lime group, among which the following were determined:[1] *Avicula* (*Pteronites*) *rugosa* Conrad; *Meristella sulcata* Vanuxem; *Leperditia alta* Conrad; Geol. of the N. Y. Fourth District, p. 142. The fossils were exhibited.

THE "EARTHQUAKE" AT NEW MADRID, MO., IN 1811, PROBABLY NOT AN EARTHQUAKE. By JAMES MACFARLANE of Towanda, Penn.

[ABSTRACT.]

AFTER dwelling upon the fact, that the locality of the alleged earthquake was not the seat of any apparent volcanic action, the author proceeded to state his view that the event in question was due to a different cause. He believed that what took place was a subsidence, due to the solution of underlying strata. He alluded to the descriptions afforded by Humboldt and Lyell, the latter having visited the locality, and given it a careful examination. The inhabitants described it as a convulsion, taking place at intervals during three years; creating new lakes and islands, changing the

[1]Since the above was written the *Spirifera Vanuxemi* Hall has been collected from the above formation.

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face of the country. The graveyard was precipitated into the Mississippi river; forest trees were tilted in all directions; vast volumes of sand and water were discharged on high.

The author presented the following among other evidence that the phenomena proceeded from mere subsidence, and not from earthquake shocks. Their long continuance, from 1811 to 1813; their progression from place to place, ending nearly one hundred miles east of where they began; that they were never repeated in the same spot; that none of the ordinary phenomena of earthquakes occurred, except subsidence; that no great alluvial region, like this, was ever visited by an earthquake; that earthquakes do not occur so far from the seashore; and that the falling in of the Wade farm of 500 acres on the shore of Reel-Foot Lake, in January, 1883, was a continuation, on a small scale, of the supposed earthquakes of 1811–13.

In respect to the geology of the region, he stated that New Madrid and its vicinity rested on tertiary or quaternary strata. Sub-carboniferous formations are represented near the borders of the depression. Many of the older formations, by which this quaternary delta must be deeply underlaid, are of a soluble character, as is shown where they are exposed in the interior of Kentucky. Similar subsidences have evidently taken place in nearly every limestone country; the remarkable thing about this being that it occurred in modern times and before eye-witnesses.[1]

[1] For discussion of this paper see Science for Sept. 7, 1883.

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Comparative strength of Minnesota and New England Granites. By N. H. Winchell of Minneapolis, Minn.

[ABSTRACT.]

Having had recent occasion to test the qualities of the building stones of Minnesota, the author subjected them to the usual tests of crushing, using for this purpose specimens of two-inch cube. The specimens included sandstones, limestones, granites and trap-rocks, and numbered about 100. Great care was taken in preparing them accurately. They were sent to Gen. Gillmore at Staten Island and there subjected to the tests, which were applied by crushing the samples, one in the direction of the schistose structure and one across it. The following were the results with twenty samples of Minnesota granites:

Kind of stone.	Location of quarry.	Position.	Strength in pounds.	
			Of sample.	Per cubic inch.
Dark trap-rock, massive melaphyr	Taylor's Falls, Chisago county	On bed.	105,000	26,250
		On edge,	105,000	26,250
Dark trap-rock, from a dyke.	Tischer's creek, near Duluth, St. Louis county,	On bed.	105,000	26,250
		On edge,	105,000	26,250
Gray gabbro, massive, fine.	Rice's Point, Duluth, St. Louis county	On bed.	109,000	27,250
		On edge,	105,000	26,250
Red, fine sienite	Beaver Bay, Lake county	On bed.	108,000	25,000
		On edge,	103,000	25,750
Red quartzose sienite	Watab, Benton county	On bed.	103,000	25,750
		On edge,	103,000[1]	25,750
Red quartzose sienite	East St. Cloud, Sherburne county	On bed.	112,000	28,000
		On edge,	105,000	26,250
Red quartzite	Pipestone City, Pipestone county	On bed.	111,000	27,750
		On edge,	108,000	27,000
Massive gray quartzose sienite	East St. Cloud, Sherburne county	On bed.	105,000	26,250
		On edge,	103,000	25,750
Fine-grained gray sienite.	East St. Cloud, Sherburne county	On bed.	112,000	28,000
		On edge,	105,000	26,250
Fine-grained gray sienite.	Sauk Rapids	On bed[2].	86,000	21,500
		On edge,	100,000	25,000
Average of	twenty samples		104,800	26,675

[1] Estimate. [2] Probably an imperfect sample.

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Allowing for eleven per cent difference between processes of crushing between steel-plates and between wooden cushions, this gives an average for Minnesota granites of 23,318 pounds per square inch.

The following are the records of tests of twenty New-England granites: —

Kind of stone.	Location of quarry.	Position.	Strength in pounds.	
			Of sample.	Per cubic inch.
Blue	Staten Island, N. Y.	On bed,	89,250	22,315
	Fox Island, Me.	— —	59,500	14,875
	Dix Island, Me.	— —	60,000	15,000
Dark	Quincy, Mass.	— —	71,000	17,750
Light	Quincy, Mass.	— —	59,000	14,750
Flagging	Hudson River, N. Y.	— —	53,700	13,425
	Cape Ann, Mass.	On bed,	59,750	14,937
Porter's rock,	Mystic River, Conn.	On bed,	72,500	18,125
Gray	Stony Creek, Conn.	On bed,	60,000	15,000
Gray	Fall River, Mass.	On bed,	63,750	15,937
Bluish-gray	Keene, N. H.	On bed,	41,000	10,250
Bluish-gray	Keene, N. H.	On bed,	51,500	12,875
	Millstone Pt., Conn.	— —	64,750	16,187
	Greenwich, Conn.	— —	45,200	11,300
Niantic river,	New London, Conn.	— —	50,000	12,500
Niantic river,	New London, Conn.	On edge,	56,700	14,175
	Vinalhaven, Me.	— —	52,600	13,150
	Vinalhaven, Me.	— —	67,000	16,750
	Westerly, R. I.	On bed,	58,750	14,689
	Westerly, R. I.	On edge,	59,750	14,937
Average of	twenty granites		59,785	14,946

After discussing several supposable causes of error, and showing that they could not have applied to the present case, the author proceeds to suggest causes why the Minnesota granites may be stronger than those of New England. He thinks those of the west may have been less changed by decay. The lateness of the glaciation to which they were exposed may have left them comparatively fresh through the recent removal of a considerable thickness. On this point we shall be more certain when the glacial moraines have been fully traced from west to east, and the eastern analogues are determined.

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THE SINGING BEACH OF MANCHESTER, MASS. By H. C. BOLTON of Hartford, Conn., and A. A. JULIEN of New York, N. Y.

[ABSTRACT.]

THE authors describe the acoustic phenomena observed by one of them at Manchester-by-the-Sea, Mass., and on the island of Eigg, Hebrides, together with the results of microscopical examination of the sands. The beach at Manchester forms a small crescent about three-quarters of a mile long, and is terminated at each end by bold promontories of granite, rich in feldspar, and intersected by numerous dykes of igneous rocks among which porphyritic diorite is noticeable. The beach sand resembles at first sight ordinary sea-shore sand, but when struck by the foot, or stroked by the hand, yields a peculiar sound which may be likened to a subdued crushing; the sound is of low intensity and pitch, and is not metallic nor crackling. This phenomenon is confined to that part of the beach lying between water-line and the loose sand above the reach of ordinary high tide. Some parts of the beach emit a louder sound than others. The sounding sand is near the surface only; at the depth of one or two feet the acoustic properties disappear, probably owing to the moisture. Only the dry sand has this property. The sounds occur when walking over the beach, increase when the sand is struck obliquely by the foot, and can be intensified by dragging over it a wooden pole or board. A slight noise is perceptible upon mere stirring by the hand, or upon plunging one finger into the sand and suddenly withdrawing it.

Similar beaches occur in various parts of the world. One of us visited the island of Eigg, Hebrides, in July and found the acoustic phenomena quite similar to those of Manchester. The Eigg locality has been described by its discoverer Hugh Miller. One of the most famous localities in the world is on the island of Kauai, of the Hawaii Islands. There the sounds are said to be so loud as to resemble distant thunder, when any great weight is dragged over it. As elsewhere dampness prevents the sound. The sand is almost wholly calcareous and has been examined by Dr. Blake of San Francisco.

In other places sonorous sand is associated with high dunes or steep hills; of these the most famous is Jebel Nakous on the Gulf

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of Suez, in Arabia Petræa, which has been described by many European travellers. A similar hill exists near Cabul in Afghanistan. The sand of these hills is silicious. In Churchill Co., Nev., a somewhat similar phenomenon is described, the sound being likened to that produced by telegraph wires when the wind blows through them.

Microscopical examination of the sands from several localities shows them to be unlike in constitution, in form, and in structure. The sand of Manchester is about fifty per cent feldspar, that of Kauai is calcareous, that of Eigg silicious. The latter is peculiar in containing dark granules of chert (to about three or four per cent of the whole) which has a cellular structure. The quartz is present in various sizes, some rounded and others angular.

To explain the sonorous properties of these sands several theories are considered. That of equality of size of the grains is rejected. Resonance due to cellular structure probably accounts for the sound in the sands of Kauai and of Eigg, but this structure does not occur in other cases. Effervescence of air between moistened surfaces does not apply to the Manchester sand. Sonorous mineral or rock, such as clinkstone, is not present. There is no evidence of electrical phenomena being concerned. The hypothesis adopted is based upon the structure of the sand which, instead of being composed of the usual rounded particles, is made up of grains with flat and angular surfaces. In the Manchester sand, the plane surface of feldspar is apparent in many of the grains. Probably a certain proportion of quartz and feldspar grains is adapted to give the sound, while less or more of either component would fail of the result.

It is concluded that different conditions are concerned in the production of the sound in different localities; as, for example, cellular structure, intermixture of grains having cleavage planes, parallel arrangement, slight cementation of the grains, etc.

The paper, which is to be regarded as a preliminary notice, ends with a table showing the physical structure of the sands from many localities. The authors add an urgent request for the contribution of similar facts and additional material from other places at which the phenomena may be observed.

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The Earth's Orographic framework: its Seismology and Geology. By Richard Owen of New Harmony, Indiana.

[ABSTRACT.]

I. *Its Orography.*

Several writers[1] have already pointed out the almost uninterrupted successions of important mountain-chains, from the Cantabrians easterly to China.

The present paper calls attention to the connection of that so-called "Stony Girdle of the Earth" with two important G. C's[2] of seismic activity, and with geological changes of comparatively recent date.

If we bring to the zenith the region of land centre, the P. V. will coincide with the main ranges of the great east and west orographic framework, at least from the Cantabrian mountains nearly to the Hindu Cush, and Pamier plateau (or "Roof of the World"), whence the P. V. crosses the basaltic Deccan to Sumatra, where it is 90° *east* of the B. M. In the same way, continuing west from the Cantabrians, we find the P. V. pass through the Azores and the Lesser Antilles, reaching Caraccas to follow the eastern cordillera of the Andes to the region of the volcanic plateau near Quito, with its three active, five dormant and twelve extinct volcanoes, exactly 90° *west* of the B. M.

A plane projected through our globe, coincident with the B. M. would divide the earth into an eastern and western hemisphere, having respectively the regions of Quito and Sumatra for the centre of each, exhibiting a duality, not so apparent when we divide the hemispheres, as in most geographies, about mid-Alantic. The unabbreviated paper points out many evidences of this duality, and indeed of a symmetrical distribution of land and water, seismic phenomena and magnetic intensity.

The Andean, and subsequently the Rocky Mountain "Backbone," or continuous chain of nearly 8000 miles (if we estimate from Patagonia to Alaska) intersects the P. V. at Quito; while the somewhat interrupted chain from Behring's Straits through

[1] Manual of Modern Geogr. by Dr. MacKay, also the "Abode of Snow" by A. Wilson.

[2] The abbreviations used in the Montreal paper "Contributions to Seismology" are here continued. B. M. (brazen meridian) is also used to indicate the meridian of land centre, where Switzerland is brought to the zenith. If an international prime meridian is ever adopted this B. M. would appear to have the strongest claims to that distinction.

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Kamchatka, the Kuriles, Japan, etc., intersects the P. V. not only near Java, in the eastern hemisphere, but also at Caraccas in the western hemisphere.

II. *The Seismology of the "Stony Girdle."*

The intersections of the P. V. at Quito, Caraccas and Java are near the regions of most internal activity, as proved by the Tomboro and other outbursts,[3] the thirty-eight volcanoes in Java, etc., on the one hand, as well as by the Cotopaxi-Sangai eruptions, and the Riobamba, Caraccas and other earthquakes of that proximity, added to the Central American Coseguina, Izalca, etc., seismic action, on the other hand.

The law seems to be that the active volcanoes often form part of the "Stony Girdle," while earthquake regions usually display their activity within areas enclosed by the great orographic links. Orton, in his "Andes," p. 161, speaking of earthquakes there, says: "Most damage is done where the sedimentary plains abut against the hard, upturned strata of the mountains."

The great arctic and antarctic volcanic chain from Kamchatka and the Kuriles, along Japan and across Sumatra, thence through the southern Pacific to South America, and along the Andean and Rocky Mountain backbone of America to Behring's Straits again, has its nearly two hundred linear volcanoes, to a considerable extent *on* the orographic main-chain; whereas when we trace the P. V. as a belt, we find its seismic regions chiefly *between* the mountain ridges which make up that belt or girdle.

Beginning at the Cantabrians, we may follow the Pyrenees, Alps, Carpathians, etc., east to the Caucasus; again we may trace a nearly parallel chain several hundred miles, south of the first, running from the Spanish Sierra Nevada, in an easterly direction through the Balearic Islands and northern Sardinia, close to Ischia, thence to the highest Balkans (some nearly 10,000 ft. high) and along Anti-Taurus to Armenia. A third parallel range may be followed from the Atlas mountains to Sicily, the Morea, and Taurus, including Candia and Cyprus, also to Armenia. . There all three unite to form the Persian Caucasus, from Demavend, along the Paropamesan range to the Hindu Cush, as already indicated.

[3] To which we may now add the recent catastrophe in the Straits of Sunda, and at the southwest extremity of Java, produced by Karakas.

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Either on, or within, this tri-partite belt, we find the extinct Auvergne volcanoes, the active mountains or other regions, such as Etna, Vesuvius, submarine "Graham," the Lipari Islands, Santorin and four other volcanic islands[4] in the Grecian Archipelago, the mud volcanoes of Perekop, the Chimæra in Lycia,[5] the Baku "Field of Fire;" also the extinct volcano of the Caucasus (Elburuz); those of Armenia (Ararat, Kasbek and Savalan Dagh; besides the active Demavend, in the Elburz or Persian Caucasus.

We have, further, within this area, the highly seismic regions of Lisbon, Switzerland and Italy, especially Calabria; also of Agram, Cattaro, and Ragusa, Negropont, Scios, Smyrna, Antioch, Tabriz in Persia, Schamaki, and numerous places, besides the Cutch, on the route to Sumatra, with its four active and fifteen extinct volcanoes.

Tracing the P. V. *west* from the Cantabrians, we have two volcanoes in the Azores and ten in the West Indies, all nearly on the P. V. line to Quito. This P. V. thus followed through the land-hemisphere, if traced through the oceanic-hemisphere, will be found to pass through volcanic New Zealand.

It is also worthy of note that when the P. V., or one of its collateral stony furcations, intersects any one of the five equidistant continental eastern trends (described in the Montreal paper on "Seismology") there are usually found important orographic and seismic results.

III. *The Geology of the "Stony Girdle."*

A close inspection of Murchison's Geological Map of Europe, as well as of D'Orbigny's "Extension Géographique," especially for the "Néocomien" and "Suessonien;" also of Dana's and Geikie's latest text-books, besides many other works detailing the Cretaceous and Tertiary areas, can scarcely fail to satisfy us that we can trace both Cretaceous and Tertiary along almost the whole line of the P. V.

The conclusion forced upon us seems to be that the Alps, the Carpathians and Balkans, the Euxine-Caspian Caucasus, as well as the Persian Caucasus, and Sivalik Hills or sub-Himalaya range, also the more southerly Atlas-Taurean strike, together with the western extension of the P. V. through the Azores and West Indies to Chimborazo, *all* owe their *present* elevation partly to the

[4] Dana's Manual, p. 703.
[5] Cosmos, Vol. iv (Sabini's trans.), p. 251.

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orographic impulse about the close of the Mesozoic, and partly to the earlier Tertiary, especially Nummulitic Eocene, and Oligocene: slowly attaining nearly their maximum eminence during the Miocene; since which little addition seems to have been made, as the Pliocene and Quaternary are usually undisturbed, except probably in the Andes. Illustrative details, which could readily be given, have of course to be omitted in an abstract.

Recapitulation.

It appears, then, that a great mountain-framework, in several parallel ranges, extends nearly east and west, from the centre of land, near the "Furka" watershed of great rivers (Rhine, Rhone, tributary of Danube, etc.) to the equator, respectively at Quito and Sumatra. Strengthening braces run from the elevated Asiatic plateau region, and sources of great diverging rivers (Brahmaputra, Ganges, Indus, with tributary Sutlej, etc.) in at least four grand ramifications through Asia; while America is similarly strengthened by its continental arctic-antarctic "backbone," whence flow the mighty waters of the Missouri, Amazon and other streams.

The east and west "Framework," and the arctic-antarctic Belt, indicate intense reaction from the interior to aid this recent, as well as earlier, mountain-making: the five equidistant continental trends often marking Paleozoic belts[6]; while the east and west "Stony Girdle" is of Mesozoic age, with its highest finish in Cenozoic times, leaving a belt of almost continuous fire, at space-intervals sufficiently near to promise a continuance of erupted products, during at least Quaternary history: namely, through the five ever active regions mentioned by Humboldt, Izalco, Masaya and Sangai; Fogo, Stromboli and Chimæra, besides others, scarcely less active such as Santorin and the Malay Archipelago

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THE CONTINENTAL TYPE, OR NORMAL OROGRAPHY AND GEOLOGY OF CONTINENTS. By RICHARD OWEN of New Harmony, Indiana.

[ABSTRACT.]

IN each one of the five great continents may readily be traced a general plan of construction, or physiography, connected with their appearance as land above the ocean; also a similarity in their geognostic history and present geological areas.

[6] See Murchison's map of Paleozoic regions, in "Siluria," to face p. 575.

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In order to make this apparent, let us construct a cross-section of each continent, taking the ocean as our base line, and running from W. S. W. through the geographical centre[1] of the continent to E. N. E.

In each case, we find a seismic belt, near at least one rim of the continent, usually of both, either on the shore border, or just outside in the ocean. Orographically, each continent is more or less basin-shaped, being comparatively low in the central area, with its chief river drainage; and low near the ocean borders; rising in an eastern and western main range, with usually several parallel subordinate ridges. These eastern and western mountains generally converge southerly, thus assuming a somewhat triangular form enclosing often on the west some table-land.

The eastern rim is usually Paleozoic with perhaps some Mesozoic on the flanks, and Cenozoic on the ocean border. The western elevation is more commonly Mesozoic in its main range, and Cenozoic on the flanks, or subordinate ridges.

A section running north and south (instead of easterly and westerly) through the three northern continents would successively expose Archæan, Paleozoic, Mesozoic and Cenozoic cuts, which formations would generally increase in area as we go south.

1. Beginning on the west with *N. America*, we find (on traversing the continent from the region of California, through the continental centre, near the west end of Lake Superior to Maine) first the seismic belt running along the west coast of N. America (marked "D′" in the Montreal paper on Seismology) then comes the Cenozoic in the coast range and on the flanks of the Sierra Nevada; subsequently the Rocky Mountains, chiefly Mesozoic; but, by their extensive basaltic outflows, forming tablelands of Tertiary areas affording land and fresh-water fossil animals, sometimes of gigantic proportions, also lignitic coal; then follows a gradual slope to the great valley of central drainage. Soon after passing the height of land, we encounter the White Mountain and Katahdin reëxtension of the Paleozoic Appalachians, with a considerable amount of Mesozoic on the eastern slopes, and some Cenozoic, rather south of our line at Cape Cod and Martha's Vine-

[1] This is usually in about lat. 48°–49° N., for northern continents; on the equator, for Africa; and on the Tropic of Cancer, for S. America and Australia; as shown in detail at pp. 437, *et seq.*, of Boston Vol. of Proc., in paper on "Law of Land-Forming."

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yard. The section closes near the meridian of the seismic belt, running from Central America, through the West Indies to Iceland: viz. Great Circle "F'."

To supplement the above east and west sections, we may trace N. America from north to south, which will give us Archæan, as far down as Lake Superior; then Paleozoic, as we go south to Tennessee; then chiefly Mesozoic (except in the extreme centre of erosion) to the state of Mississippi; and thence Cenozoic, to the Gulf of Mexico, as well as in Texas and Mexico.

2. Let us take *Europe* next, in cross-section from northern Spain, through Tatra (the centre of the continent) to the Caspian, its eastern termination, in a depression 84 ft. lower than the ocean. We find on the west a seismic belt (G. C. " C.", passing over the submarine volcanic areas of the Cape Verd and Canary Islands, besides Lisbon); then some Cenozoic in north Spain; then chiefly Mesozoic with some Cenozoic in the Pyrenees, Alps and Carpathians; then a valley of great drainage: the Danube, Dniester, Dnieper, Don and other rivers flowing into the Black Sea; besides the Volga, etc., into the Caspian.

On or near this section line, we have trap, trachyte and other ancient eruptive rocks, flowing out at various points in France, Switzerland and Austria; then we traverse the tablelands of Hungary and Transylvania. Although the Caucasus is chiefly Mesozoic with Cenozoic on the flanks, yet a little south of our section line, Murchison shows a considerable area of Metamorphic, and some Paleozoic, along the anti-Taurus mountains, as they trend toward Armenia; while north of our section, we have the Paleozoic of Perm and the Urals, with outflows of trap, extending nearly the whole length of that range; and lastly we encounter the highly seismic regions around the Caspian, which form part of G. C. "D.," the same which passes along our Appalachians.

3. *Asia.* In a cross-section from the Caspian through the continental centre, near Lake Baikal, to Japan, starting from the low, seismic region, we cross portions of the radiating ridge from the Hindu Cush and Himalayas; then traverse the comparatively low Lake Baikal region, and meet the Khingan mountains on our route to Japan.

The high Himalayas are at least partly Mesozoic, with Cenozoic on their flanks, in the Sivaliks; the central portion of Asia is also partly Mesozoic, partly Cenozoic; in the Provinces between the

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south extremity of the Khingans and the eastern ramifications of the Kuen Lun, namely, in Hunan and Szechnan,[2] the Paleozoic affords true Carboniferous coal; while in Japan with its numerous volcanoes, there is much Cenozoic.[3]

4. *S. America.* The centre of this continent being on the Tropic of Capricorn, our section may run from the region of N. Chili, through Brazil, a little north of Rio. In this section, we have the Cordilleras in double range, and find Sorata and Sahama, the highest mountains in the western hemisphere, somewhat north of this line. The mean height of the Brazil tablelands is from 2000 to 3000 ft.; but the eastern Sierra Montequeira, north of Rio, attains the altitude of 10,000 ft.

The great centre is drained north by the Madera river, running into the Amazon; south by the Paraguay and Parana flowing into the La Plata.

With reference to the geology of this region, Darwin, D'Orbigny, Professors Hitchcock and Orton[4] mention considerable areas of Paleozoic in the Bolivian Andes and in eastern Brazil, with some intermediate Oolitic, Cretaceous and Tertiary; the latter extending, with Quaternary, south over much of the Argentine Republic and all Patagonia. They think that this Cenozoic occupies the area of wide straits, which formerly separated the Andes from Brazil. Some Paleozoic rocks are also found in Chili, along with its enormous mountains of trachyte and active volcanoes. Erupted porphyries are likewise very abundant. On each side of the continent there are seismic belts; one on the east partly submarine (G. C. "C"), the other on the west, running along the Andes, etc., (G. C. "D") indicating the normal type.

5. In *Africa*, as the centre of land is near St. Thomas's Island, it is more convenient to run the section a little farther north, say from Liberia, nearly through Lake Tchad to northern Abyssinia, so as to give land, instead of water, on the western half. The law here holds good; showing us the elevations of the Kong mountains to some 9000 ft., and at the north, in the distance, the Atlas range attaining over 11,000 ft., while the more eastern ranges in Abyssinia reach 12–15,000 ft. We observe also the extensive central depressions, and the drainage south by the Niger.

The geology is not accurately known yet, but seems to be Cen-

[2] See Thompson's China, p. 441. [3] Miss Bird's Japan. [4] "The Andes," p. 117.

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ozoic with some Mesozoic centrally (especially farther north in Sahara); and, outside the eastern basin rim, chiefly Cenozoic in Arabia and around the Persian Gulf. The Atlas mountains are Cretaceous, overlying Paleozoic, with some Cenozoic on their flanks. Along the Abyssinian Metamorphic mountains, with some Triassic, we trace, as also in Ethiopia and Egypt, dioritic and syenitic eruptions. The whole of the Pyramid of Cheops is of nummulitic limestone, but the lesser pyramid, beyond the Sphinx from Cheops, has its first layer of syenite, the next being nummulitic limestone. The Sphinx itself is carved from a red sandstone *in situ*, partly covered by the drifting sand, but still showing distinctly the beds of stratification.

Outside the continent of Africa, we have two great seismic belts; that on the west being G. C. "C," which embraces the volcanoes of Cape Verd, Canary Islands, etc. The seismic G. C. "B," on the east, besides traversing the Sandwich and Comoro Islands, runs near the Bourbon and the Red Sea volcanoes and crosses the volcanic and earthquake region of Central Asia. The existence of the Thian Shan volcanoes, notwithstanding the negative, opposing hearsay evidence of Schuyler, in his work on Turkestan that "no volcanoes had been seen by his informants," nevertheless receives strong positive testimony, in Chinese records; and has secured the sanction of Humboldt, Mallet, Guyot and other eminent scientists.

On Rhizocarps in the Palæozoic Period. By J. W. Dawson of Montreal, Canada.

[ABSTRACT.]

The paper noticed the discovery of *Sporangites Huronensis* Dawson, in the Erian shale of Kettle Point, Lake Huron, and its description by the author in 1871,[1] when these organisms, abounding in a bed of bituminous shale twelve feet in thickness, were described as probably sporangia or macrospores of some acrogenous plant. Mention was then made of the subsequent discoveries of Professor Orton in the Erian shales of Ohio, and those of Prof. H. S. Williams and Prof. I. M. Clarke in New York, whereby it

[1] American Journal of Science.

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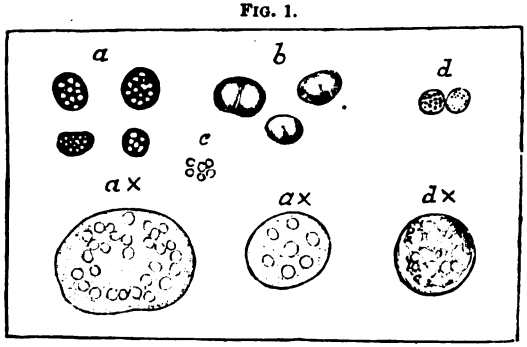
appeared that similar organisms are abundantly distributed in those states through shales 250 to 300 feet thick, and extending over wide areas. The late Professor Hartt had also found similar bodies in Brazil. A new light had been thrown on these facts by the discovery in the Erian shales of Brazil of these bodies, still contained in circular envelopes or sporocarps similar to those of *Salvinia* and other modern Rhizocarps. This discovery was due to Professor Orville Derby of the Brazilian Geological Survey, who also had sent specimens to the author.

Mr. Derby's specimens contain *Spirophyton* and also minute rounded *Sporangites* like those formerly obtained by Professor Hartt. But they differ in showing the remarkable fact that these rounded bodies are enclosed in considerable numbers in spherical and oval sacs, the walls of which are composed of a tissue of hexagonal cells, and which resemble in every respect the involucres or spore-sacs of the little group of modern acrogens, known as Rhizocarps, and living in shallow water. More especially they resemble the sporocarps of the genus *Salvinia*. This fact opens up an entirely new field of investigation as to these interesting specimens, their probable affinities with modern plants and their probable relations to other palæozoic forms of vegetation found with them.

Mr. Derby's specimens are labelled as from Rio Trombetos and Rio Curuá. They occur in two kinds of matrix. One is a thinly

FIG. 1.



(*a*) *Sporangites Brazilensis*, nat. size. (*a* ×) same, magnified.
(*b*) *Sporangites bilobatus*, nat. size.
(*c*) Detached macrospores.
(*d*) Spore-cases of *Salvinia natans*. (*d* ×) same magnified.

laminated sandy shale, tinged red with peroxide of iron, and with occasional ferruginous laminæ. In this the spore-sacs are flattened and black, and show the structure of the walls under the

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microscope. The contained macrospores, when visible, appear as minute tubercles or sometimes as depressions on the wall of the envelope, or more frequently as round light colored spots, according to their state of preservation (fig. 1, *a*). The other kind of matrix is a gray dense shale in which the spore-sacs appear less flattened and destitute of carbonaceous matter.

The very numerous spore-sacs contained in these shales are extremely variable in size and form, and may have belonged to several species of plants. They resolve themselves, however, into two leading types, which may be provisionally named *Sporangites Braziliensis* and *S. bilobatus;* though I would suggest for them, in prospect of the discovery of their vegetative parts, the name *Protosalvinia*. They may be described as follows:—

Sporangites (*Protosalvinia*) *Braziliensis*, s. n. (fig. 1, *a*). Sporocarps thin,. carbonaceous, having a structure of dense hexagonal cells and enclosing macrospores which vary in number from three or four to as many as twenty-five. Form circular, oval or reniform. Longest diameter from three millimetres to six millimetres. Some of the sacs which do not show included round bodies may have held microspores.

S. (*P.*) *bilobatus*, s. n. (fig. 1, *b*). Sporocarps oval or reniform, three millimetres to six millimetres in diameter, each showing two rounded prominences at the ends, with a depression in the middle, and sometimes a raised neck or isthmus at one side connecting the prominences. Some of the specimens indicate that each prominence or tubercle contained several macrospores. At first sight it would be easy to mistake these bodies for valves of *Beyrichia*.

The following remarks were made on the probable affinities of these bodies and their relation to other Erian plants:

There can be no question of the close resemblance of the Brazilian species of *Sporangites* with the spore-envelopes of modern Rhizocarps. Some individuals of the *S. Braziliensis* are scarcely distinguishable in form or contained macrospores from the sporocarps of *Salvinia natans* of the rivers of Europe (fig. 1, *d*). It is true that the analogy of *Salvinia* would lead us to expect other sacs containing microspores; but in ordinary circumstances the latter could not be preserved in a visible state, and on the Brazilian shales there are many specimens not showing macrospores, and which might have been filled with microspores which have been flattened into an undistinguishable mass.

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If we compare the separate macrospores of the Brazilian sporocarps, and especially those which are found detached from their envelopes, with *Sporangites Huronensis*, we see a remarkable similarity in size, form and texture, sufficient to justify us in supposing that the latter may be of the same nature with the former, but deprived of their outer cases either by dehiscence or by decay, and this is the view which I am now disposed to take of their nature, and which better accords with their wide distribution in aqueous deposits and with their accompaniments than any other supposition. I may add that Professor Orton and Professor Clarke, in letters to the author, refer to grouping of the little rounded bodies and traces of enveloping membrane. In this connection I would also mention the sacs containing rounded bodies known as *Parka*, and which have been met with in the Erian beds both in Scotland and in Gaspé, and have been supposed to be ova of crustaceans. It is true that these are much larger than the sporocarps above referred to, but on examination of Gaspé specimens in my collection, I am disposed to suspect that they also may prove to be the fructification of Rhizocarps.

It remains to enquire, Are there any Erian plants known to us in their stems and foliage, to which such organs of fructification as those above described might have belonged?

A preliminary question would naturally be as to the vegetative organs of modern Rhizocarps. On reference to the descriptions, and to a somewhat extensive collection of specimens placed at my disposal by Mr. Poe Watt of Montreal, I find that these may be referred to three leading types. Some, like *Pilularia*, have simple linear leaves; others, like *Marsilea*, have leaves in verticils and cuneate in form; while others, like *Azolla* and *Salvinia*, have frondose leaves, more or less pinnate in their arrangement. The first type presents little that is characteristic, but there are in the Erian sandstones and shales great quantities of filamentous and linear objects which it has been impossible to refer to any genus, and which might have belonged to plants of the type of *Pilularia*. It is quite possible also that such plants as *Psilophyton glabrum* and *Cordaites angustifolia*, of which the fructification is quite unknown, may have been allied to Rhizocarps. With regard to the verticillate type we are at once reminded of *Sphenophyllum*, which many palæo-botanists have referred to the *Marsiliaceæ*, though like other palæozoic Acrogens it presents complexities not seen in its modern

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representatives. *S. primævum* of Lesquereux is found in the Hudson River group, and my *S. antiquum* in the middle Erian. Besides these there are in the Silurian and Erian beds, plants with verticillate leaves which have been placed with the *Annulariæ*, but which may have differed from them in fructification. *Annularia laxa* of the Erian and *Protannularia Harknessii* of the Silurian may be given as examples. As to pinnate leaves, I have already referred to the remarkable plants of the genus *Ptilophyton*, found both in the Erian and Carboniferous, and which seem to have been aquatic in their habit like Rhizocarps. It is deserving of notice also that the two best known species of *Psilophyton* (*P. princeps* and *P. robustius*), while allied to Lycopods by the structure of the stem and such rudimentary foliage as they possess, are also allied, by the form of their fructification, to the Rhizocarps, and not to ferns as some palæo-botanists have incorrectly supposed.[1]

I do not suppose that the facts above stated furnish any positive proof that the abundant *Sporangites* of the Erian period were the fructification of Rhizocarps, but they establish a certain probability of this, and invite to further researches. If it should prove that these humble plants, now so insignificant, culminated in the Palæozoic age, and occupied the extensive submerged flats of that period with an abundant vegetation, producing a great quantity of the bituminous matter found in the resulting beds, this early culmination of the Rhizocarps would be strictly in accordance with other facts in the development of the vegetable kingdom. We may even be permitted to speculate on the existence in the early Palæozoic and Eozoic ages of a rich Rhizocarpean vegetation, anticipating the great development of the Acrogens in the later Palæozoic.

I have not referred above to the well-known fact that in certain beds of coal and shale of the Carboniférous period there are multitudes of globular spore-cases or macrospores not dissimilar from those above described. These may have been derived from plants of higher organization than the Rhizocarps, yet it is quite possible that this group of plants may have contributed to them. It is, however, only in the Erian that these *Sporangites* are so widely and abundantly distributed in aquatic beds, and that we have direct evidence as to their origin.

[1] See Report on Erian Plants of Canada, 1882.

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On a large Crustacean from the Catskill Group of Pennsylvania. By E. W. Claypole of Buchtel College, Akron, Ohio.

In this paper an account was given of a very large crustacean fossil recently found in the Catskill sandstone of Pennsylvania. Only the head remains and part of that is incomplete. One side however is perfect and the other can therefore be readily restored. In shape it is semi-elliptical, resembling one end of an ellipse. Roughly speaking, it bears a striking likeness to a vastly magnified head of Cephalaspis. But the total absence of all traces of bone excludes the possibility of its being ichthyic. The remains of the creature consist of a thin black scale covering the whole surface except where it has been removed by accident or compression; a longitudinal ridge extends along the middle, rising apparently at both ends into spines or tubercles and another ridge crosses this near its hind end. This ridge is continued in a double curve resembling the arms of an anchor fore and aft.

So far as I have yet been able to make a study of this fossil, it appears to be more readily allied to the king crab (*Limulus*) than to any other existing order. But its immense size, 8½ inches in both directions, renders it very conspicuous and the elongated outline of its head is in strong contradistinction to the semicircular head-thorax of *Limulus*. There can be little doubt that it was closely allied to the great fossil crustaceans of the order Eurypterids. But the squarish outline of the head of these is totally wanting and instead of the flat smooth glabella the surface of the head is ridged as above described.

I have been unable to discover any trace of eyes and am therefore unable to obtain the valuable evidence they would give in placing the fossil. But the addition of so strange a form to the meagre fauna of the Catskill, from which only three or four fish have thus far been described, raises the hope that one day we may become better acquainted with the life of the Catskill age of which at present almost nothing is known.

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Rensselaeria from the Hamilton Group of Pennsylvania.

By E. W. Claypole of Buchtel College, Akron, O.

[ABSTRACT.]

The genus Rensselaeria has been hitherto confined almost exclusively to two horizons or groups: the Lower Helderberg and the Oriskany sandstone. Of the twelve species thus far described, four have been obtained from the former and seven from the latter of these groups.

One only, *R. Johanni*, has been described by Prof. Hall from the Corniferous rocks of Waterloo, Iowa. The specimens have all the outward appearance of Rensselaeria, but Prof. Hall expresses some doubt concerning the accuracy of his determination.[1]

The occurrence therefore of well marked large specimens of Rensselaeria high up in the Hamilton rocks is a fact deserving of note, being at least unexpected.

In regard to the species I must say that I am at present quite unable to separate it from *R. Marylandica* of the Oriskany sandstone.

The material in which it occurs is a coarse sandstone known in the geology of Perry County as the Hamilton Sandstone, a local deposit in the group occurring about 800 feet above the horizon of the Marcellus black shale and about 1000 feet above the Oriskany Sandstone. Its only accompanying fossil is a spirifer closely resembling, and I believe identical with, *Spirifera formosa*, Hall.

Preliminary Note on the Microscopic Shell-structure of the Paleozoic Brachiopoda.

By James Hall of Albany, N. Y.

[ABSTRACT.]

In the earlier studies of the fossil Brachiopoda, the numerous species were referred to few generic terms, determined mainly from their perforate or non-perforate apex and external form, and later from the study of the interior as these parts became known. The author said, that from time to time, as these characters had become known to him from the study of large collections, he had found it necessary to propose the separation of more than twenty new gen-

[1] Professor Hall informs me that he has now removed the species in question from this genus.

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eric forms beyond those previously recognized in this class of fossils. Other authors had also proposed new generic terms, until the list had become many times greater than it was twenty-five years ago.

While the interior structure of the hinge, and the muscular and vascular markings, were now pretty well known for most of the generic forms in use, comparatively little attention had been given to the minute structure of the shell.

The study of this structure had been commenced by him many years ago, but he had been thwarted in his efforts to procure the required cutting and polishing of specimens of the shells necessary for microscopic study. Through the aid of Mr. C. E. Beecher, Assistant in the State Museum of Natural History, he had now been able to obtain such thin slices of the shell as were required for this purpose, and had already several hundred slides prepared for the microscope.

A few of these only were shown, exhibiting the shell-structure of as many genera. A considerable number of photographs had been made, illustrating, in a very satisfactory manner the minute structure of each one, enlarged to twenty diameters. The photographs exhibited were illustrations of several species of ORTHIS, LEPTÆNA, STROPHOMENA, STROPHODONTA, CHONETES, etc.

The study of this shell-structure has shown, very satisfactorily, what was partially known before, that the genus ORTHIS, as now defined and constituted, includes very heterogenous material. External form, hinge characters, and interior muscular impressions, have been the chief guide; and yet forms have been included under this genus, showing widely different muscular markings. On further microscopic study, it has been found that these differences in form of muscular impressions are accompanied by important differences in the shell-structure.

These differences may be noted in the illustrations presented, where the shell of *Orthis biforata*, *O. borealis*, *O. tricenaria*, *O. occidentalis*, *O. flabella*, are non-punctate and coarsely fibrous, *Orthis* (?) *strophomenoides* is, like STREPTORHYNCHUS, fibrous. *Orthis subquadrata* has, like *O. occidentalis*, a few large punctæ.

In the second group, *Orthis testudinaria*, *O. Vanuxemi*, *O. perveta*, *O. penelope*, *O. elegantula*, *O. clytie*, and *O. hybrida*, have one or more rows of punctæ to each ray, the rows well defined, and the intermediate shell finely fibrous.

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The third group, consisting of *Orthis multicostata* of the lower Helderberg, *O. Iowensis* of the Hamilton group, *O. Tulliensis* of the Tully limestone, *O. impressa* of the Chemung group, are highly punctate with a fine fibrous texture of the shell substance.

The punctæ usually come out along the summits of the radiating striæ or plications of the shell. In some species the minute tubes perforating the shell, and producing these punctæ, bifurcate and diverge before coming to the external surface of the shell.

This difference in shell-structure, in forms known as ORTHIS, will require a separation of the species into groups based upon this structure, and character of muscular impressions. Already we see that the shells of compact fibrous texture have a form of muscular impression quite unlike those with the punctate structure; and we shall probably find that all the interior modifications of the muscular system are accompanied by differences in the microscopic structure.

This method of determining the shell-structure, in cases where the specimen may be imperfect, and thereby enabling the determination of obscure or fragmentary material, and its geological relations, will be of much importance to the geologist.

The structure of the shell in STROPHOMENA is closely fibrous, with distant large punctæ. In STROPHODONTA, the punctæ are more numerous. In CHONETES, the punctæ are large and arranged in a single row upon the radii, having a pustulose aspect similar to those of STROPHODONTA.

In many other forms, the punctate texture of the shell is characteristic, and of importance in the determination of the generic forms.

The physiological significance of this peculiar shell-structure will be considered upon some future occasion, illustrated by more numerous examples.

ANIMAL REMAINS FROM THE LOESS AND GLACIAL CLAYS. By WILLIAM MCADAMS of Alton, Ill.

[ABSTRACT.]

THE drift clays proper at Alton, Ill., had a maximum thickness of about one hundred feet, and the bluff clays were nearly of the

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same thickness. These clays were remarkably rich in animal remains, such as teeth and bones, attached to calcareous nodules or claystones. Remains of thirteen different species, now perhaps all extinct, had been found. The rodents were well represented by bones of seven species, including three or more beavers and some gophers. Nearly seventy teeth were found in the quaternary deposits, a majority of them in a single quarry.

A New Vertebrate from the St. Louis Limestone. By William McAdams of Alton, Ill.

[ABSTRACT.]

One of the groups of subcarboniferous limestones is quarried extensively near Alton and St. Louis. It lies beneath the coal, and in some places the coal rests directly upon it. A number of vertebrate remains have been found in one of the quarries near Alton. Specimens were shown to the Section. In the judgment of Professor Newberry, the fossils shown were the bones of some large fish. One appeared to be the mandible or dental bone of the lower jaw. Without pronouncing a final opinion, he would say that it bore a general resemblance to a group of fossil fishes in which the teeth were inserted in sockets; but the animal itself was large and hitherto unknown.

On a White Garnet from near Hull, Canada. By George F. Kunz of New York, N. Y.

[ABSTRACT.]

Twenty-one miles north of Hull on the Gatineau river, at Wakefield, Canada, in a seam of crystalline magnesian limestone are found some remarkable white garnets. This occurrence has been known to a number of collectors for a few years, but as yet no information regarding them has been published. I found, on visiting the locality, that they form a vein from several inches to over one foot in width in magnesian limestone, the vein running east and west, a distance of over seventy-five feet. The garnets are entirely embedded in the limestone which can best be removed by the action of acid not affecting the crystals.

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This vein was followed to the depth of more than six feet. The crystals vary from the size of a pin-head to three inches across.

The color is from colorless and transparent (small mineralogical gems having been cut from them) to yellow running into brown.

This locality is possibly the only one that has furnished white garnet as a gem. The latter color is very often the result of the oxidation of the pyrrhotite. Associated with the garnet are crystals of pyrrhotite and fine crystals of a white pyroxene; the crystals adhering together, are held together sufficiently by the pyroxene to form fine groups of this mineral when the limestone has been removed by the acid. Perfect isolated crystals are very uncommon. The specific gravity of the mineral is 3.6002 and 3.5948 (C. Bullman); 3.52, 3.53 (T. S. Hunt, Orford, Canada).

The Orford composition is almost identical with an analysis of a white garnet by T. Sterry Hunt, from Geological Survey, Orford, Canada, 1863, p. 496.

C. BULLMAN.		T. STERRY HUNT
Silica,	38.80	38.60
Alumina,	22.66	22.71
Sesquioxide Iron,	1.75	
Oxide Manganese,	0.30	
Lime,	35.00	34.83
Magnesia,	0.08	0.49
Oxide Iron and Manganese,		1.60
Potash,		Trace.
Soda,		0.47
Ignition,		1.10
	99.19	99.80

Spectroscope gave no potassium lime. Soda not determined.

The forms observed were 1, and 1 and 2–2 combined.

ANDALUSITE FROM A NEW AMERICAN LOCALITY. By GEORGE F. KUNZ of New York, N. Y.

[ABSTRACT.]

AT Gorham, Me., on the shores of Sebago lake, about one year ago, some excavating was done for a road, and from the pile of rock thrown out in blasting a mass was selected by Mr. Hayden of Raymond, Me., who resides in the vicinity, who thought from the outward appearance and the occurrence in it of pyrrhotite in crystals that it might contain something of interest. This sometime later, he found to contain the specimens now presented here.

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These crystals, for perfection, in color and size, are equal to those found at any known locality where this mineral does not occur as a gem. The color is mostly brownish flesh-color and at times the pink color fades to a faint greyish pink. The crystals are opaque but translucent in pieces of from 3mm. to 5mm. in thickness; specific gravity 3.12mm.-3.14mm.; size, a broken crystal measured 90mm. (long), 53mm. (broad) 48mm. (thick).

Several measure over 80mm. in length and one fragment of a large crystal measured 55mm. on one face and may have originally measured over 70mm. on the macro-prism. Some crystals are very small, say 1mm. in diameter.

The hardness on the side of the prism is from 6-6.5, on the O plane 7-7.5. The crystals consist of a simple orthorhombic prism with the O plane largely developed, the other faces have not been fully determined as yet, but are possibly I $\breve{i}$, I $\bar{i}$.

The crystals occur in a quartzite vein in a mica schist of a brown color scattered through which are beautiful small crystals of pyrrhotite. The crystals which I here exhibit and a few fragments are all that have been found as yet in the locality. A hasty visit paid to the locality failed to bring to light any more specimens, the rock having been thrown back into the excavation and all traces of mineral thus obliterated. More attention paid to this locality would surely repay the mineralogist with a good reward, as minerals of this class are rarely found in a few large isolated crystals, when the indications are so favorable as in this case.

Topaz and Associated Minerals from Stoneham, Oxford County, Maine.[1] By George F. Kunz of New York, N. Y.

[ABSTRACT.]

Associated with topaz that exceed in size and perfection those as yet found at any American locality, I found a number that are worthy of mention. The topazes were in perfect crystals. The following forms have been observed: O, $i\text{-}\bar{i}$; prisms I, $i\text{-}\breve{\frac{2}{}}$, $i\text{-}\breve{2}$, $i\text{-}\breve{3}$, $i\text{-}\breve{4}$; macrodomes $\frac{2}{3}\text{-}\bar{i}$, $2\text{-}\bar{i}$; brachydomes $2\text{-}\breve{i}$, $4\text{-}\breve{i}$; pyramids $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{3}$, 1, 2; $\frac{4}{3}\text{-}\breve{2}$, $2\text{-}\breve{2}$.

[1] In a lot of minerals in the possession of N. H. Perry of South Paris, Maine, August 1882, I observed a crystal that I at once recognized as topaz. Since then considerable work has been done by us, and the minerals here described brought to light. To Mr. Perry much credit is due for his observation and careful manner of working.

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The finest crystals were from 10mm. to 65mm. long; the largest crystal measured *a*, 60mm., *b*, 65mm., *c*, 56mm., and was entirely perfect and transparent in parts. The color of the crystals varied from colorless to light green and light blue. Small perfect gems have been cut from some of the fragments.

The large rough crystals measured from 200mm. to 300mm. across, and weighed from 10 to 20 kilos; faces rough and the crystals broken in the bed; color of these light blue, light green, and white or yellowish.

These crystals are full of the characteristic fluid cavities. The hardness same as the Brazilian.

Columbite was scattered through the cleavelandite in crystals from 1mm. to 10mm. in length. A number of acicular crystals bunched together resembled a fibrous structure.

One pocket contained about 20 kilos of this mineral, and one uncouth part of a crystal weighed 8.0 kilos; evidently the whole mass was either a crystal or a group of crystals.

Triplite is scattered all through the rock in pieces from one gramme to one kilo in weight, staining all the other minerals, especially the quartz and cleavelandite.

One mass that may have been a large rough crystal furnished over fifty kilos of this mineral. The color is a light chocolate brown, streaked with almost transparent veins of a lighter colored variety of the same mineral, and is remarkably pure in character, translucent in small pieces, possibly the Var Zweiselite.

Triphyllite, one very imperfect crystal, was found about 15mm. long.

Montmorillonite in pink masses, shading from light to dark pink in color, filling cavities between crystals and plates of cleavelandite, these impressions having an apparent crystalline form; also in small botryoidal masses, resembling rhodocrocite identical with the Branchville mineral described by Brush and Dana.

Uranite in minute scales on the cleavelandite was observed.

Dendrites cover the cleavage faces of the felspars.

Beryl occurs in huge crystals through the entire hill and almost in contact with the topaz. One vein was traced for over forty feet that was full of crystals several feet long and about one foot across. No fine crystals were found here.

Zircon crystals are scattered through the cleavelandite, from 1mm. to 10mm. in length. The faces are all dull and sometimes

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the crystals are altered to malacon; faces observed were: Pyramid 1. Prism I, *ii*.

Garnet in poor crystals occurs sparingly; it is evidently a manganesian variety and resembles the triplite in color.

Cleavelandite in fine, large plates and cleavages, by the ton, is of a snowy white color or turns to this color, from a dark brown, after a short exposure to the action of the sun. Pockets in it are, as elsewhere, lined with beautiful minerals. In and at the juncture with the mica the fine topazes were found. The color is due to the decomposition of the triplite.

Quartz occurs in abundance, massive and milky in color, breaking with an apparent cleavage, at times curiously spotted with triplite.

Small, irregular crystals, and one smoky crystal 80mm. by 75mm., were found. The massive variety is filled with fluid cavities, and large pieces break easily as if with apparent explosion.

Apatite, small, very perfect crystals, white in the centre, and blue, purple or green at each termination. Faces observed: Pyramid 1. O largely developed. Prism I and *i*2.

A massive, compact, vitreous, dark green variety in nodules weighing one kilo each.

Fluorite fills cavities in the cleavelandite, dark purple in color; and some small, beautiful octohedral crystals, of a light blue color, resembled topaz in appearance.

Margarodite and muscovite in large quantities, forming veins; the former light yellowish green almost radiated in structure, the latter cleaving in fibrous masses and then in crystals transparent through the prism, although two to six inches in diameter.

Curved mica Damourite? in large curved shells resembling saucers, some of them four inches across.

Biotite crystals crystallize in the muscovite and the color of very thin sections is a dark brown by sunlight.

These minerals occur in a coarse granite hill (250 feet long and about 100 feet wide at the top), on Harndon Hill, Stoneham, Me., near the Stow line, all the constituent minerals crystallizing on a large scale. Work may be resumed at an early date and may bring some very interesting things to light.

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Colored Tourmalines and Lepidolite Crystals from a new American Locality. By George F. Kunz of New York, N. Y.

[ABSTRACT.]

In addition to Paris, Hebron and Norway, Maine, as localities for colored tourmalines, I will now briefly describe a new locality, *i. e.*, Auburn, Oxford, Maine.

Here, associated with smoky quartz, orthoclase, amblygonite, columbite, lepidolite, muscovite, cleavelandite, beautiful blue, pink, green and purple apatite, cassiterite and zircon, were found a number of pockets of fine colored tourmalines. The colors of the crystals obtained were red, blue, green and colorless, and at times all three colors in different parts of the same crystal as at Paris, Maine. The crystals, however, differ in general appearance and also in form, and as a rule are lighter in color. The largest crystal of tourmaline was 105mm. long (over 4 inches), and 10mm. wide, transparent, but flawed, and had all the colors combined, red, green, blue and colorless, in different parts. Crystals from 10 to 60mm. were found, and the larger number were terminated by a pink cap. A number of gems were obtained, principally colorless, green pink and blue.

Forms observed: Pyramid $\frac{O}{-\frac{1}{2}R}$ Prism 1 *ii*

Smoky quartz in crystals 150mm. to 200mm. in length, coated with a curious felspathic mineral, and at times penetrated by the colored tourmalines for several inches.

Lepidolite is found here in hexagonal, isolated crystals, crystallizing out in distinct crystals on larger ones of the same mineral or on crystals of quartz and feldspar, although not sharply enough defined for measurement, yet they are the finest examples of this mineral in crystal form that I have as yet observed from any locality.

In size they are from 5 to 8mm. wide, and from 5 to 15mm. in length. Large masses have parts of crystals showing that they are from 30mm. to 60mm. in length.

The interior of the crystals is a fine pink color, but the outside is a light pink with a silvery lustre. The other minerals are not

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worthy of any special mention, except the columbite, of which a very fine perfect crystal was found, and that the muscovite alters to lepidolite. This locality was first worked and the minerals found in place by N. H. Perry of South Paris, Maine, to whose kindness I am indebted for information and use of specimens.

A Note on the finding of two fine American Beryls. By George F. Kunz of New York, N. Y.

[ABSTRACT.]

In addition to the topaz and other minerals from Stoneham, Oxford Co., Maine, the finding of two beryls, exceptional for American occurrence, may be mentioned. These were found several miles apart, and several miles from the topaz localities. They were discovered by chance within the last two years, by farmers, in pastures in this township. The first is only one-half of the original crystal and has been roughly used by some one who, possibly, discarded it as worthless after breaking, or else broke it in taking from the rock and then rejected it, not knowing its value.

This crystal is 120mm. long and 54mm. wide, and was evidently 130mm. long and 75mm. wide. The color is a rich sea-green as viewed in the direction of the longer axis of the prism and sea-blue of a very deep tint through the side of the crystal. The color and the material in this crystal are the finest that have been found at any American locality and, if not broken, the crystal and material would be equal to the finest foreign specimens known. It will, if cut, furnish the finest large gem of this mineral found in the United States, one gem weighing at least thirty karats, and a number of small ones weighing from one to six karats.

The other crystal is doubly terminated, being 41mm. long and 15mm. in diameter. Over one-half is transparent with a faint green color, the remainder being of a milky green and only translucent. At the juncture of the two colors the crystal has the ap-

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pearance of a solution in which a flocculent precipitate has almost completely settled leaving the upper portion nearly clear.

The observed planes are, O, largely developed, 1 and 2-2; prism, I and i-2. The finding of the two crystals in such a manner can but lead one to think that rich material must be stored in the vicinity and would warrant farther search.

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SECTION F.

BIOLOGY.

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ADDRESS

BY

PROFESSOR W. J. BEAL,

VICE PRESIDENT SECTION F.

AGRICULTURE: ITS NEEDS AND OPPORTUNITIES.

INSTEAD of presenting a summary of the progress made in biology during the past year, I have chosen, rather, to speak of the "Needs and Opportunities of Agriculture," — a subject that has heretofore scarcely been mentioned at the meetings of this association. Within the past few years the progress of agriculture, which I use in its broadest sense, has been greater than ever before. This may be attributed to a variety of causes; such as the general thrift and intelligence of our people, and the advancement of science.

Many agricultural schools have been established, experiment-stations organized, the rural press has been much improved in quality and quantity, clubs and societies are flourishing, and thousands of granges have helped to stimulate thought and investigation. Though there is much chance for improvement, the U. S. Department of Agriculture in several of its sections has done excellent work. It is true, and it is strange that it should be true, that, until within a comparatively recent period, but very little of the best thought, even of civilized nations, has been devoted to subjects intended to advance agriculture.

Columella, eighteen hundred years ago, keenly felt the want of more thought in agriculture when he said, "Husbandry alone, which, without doubt, is next to, and, as it were, near akin to, wisdom, is in want of both masters and scholars. Of agriculture, I have never known any who professed themselves either teachers or students." Many of our states have freely appropriated money to conduct surveys in geology, mining, with a little attention

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given to zoölogy and botany, not neglecting to provide liberally for coast-surveys.

The nation, considering its age, has also been very generous with money in support of surveys of various kinds, including also, anthropology, construction of lighthouses, improving rivers and harbors, investigating the supplies of fish, and even astronomy has been generously supported. It is true that some of this work performed by the government has been very poorly done, and has been enormously expensive; but the methods of work are improving.

This munificence of the United States in support of science is encouraging, and, as far as it goes, speaks well for the country and our law-makers. Doubtless, in many cases, the close connection with politics is one great hindrance to conducting successfully investigations in science for the government. The chances of losing positions are often too great to make them desirable, especially to persons who dislike political contests. Frequent changes are fatal to good, long-continued work.

Notwithstanding the large sums of money expended by our national and state governments in support of science, but a small sum, considering its importance, has been appropriated in the interest of agriculture. Even private gifts have gone to endow literary colleges, schools of physical or natural science, astronomical observatories, public libraries, and not to endow something which is directly intended to encourage agriculture. The men like Lawrence, Sheffield, Smithson, Peabody, Washburn, Swift, Stevens, are numerous, but not numerous enough. All honor to the noble names of those who have so generously contributed to the advancement of science!

To illustrate the hesitancy of men to bequeath money for the promotion of agriculture, I take the following from an address given by President T. C. Abbot:—

"I met a very pleasant and intelligent gentleman, who, from his large wealth, was about to give some sixty or seventy thousand dollars for the advancement of higher education. He had been for some years, and was still, the president of a state agricultural society. He was a farmer. Did he then endow some chair of agriculture, or agricultural chemistry, of veterinary science or horticulture? Did he fit out an experiment-station to analyze fertilizers, to study the value of cattle-foods? Did he establish an agricultural

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library? None of these. He found the science that was the most advanced of any, the one that government supports at a great expense from the public treasury. This farmer gave his thousands to endow another workshop of astronomy."

Yet, even in respect to private endowment, there are approaching signs of better days for agriculture. A few far-seeing men have observed the needs of this interest, and have set a noble example by giving of their wealth bounteously. Cornell, Bussey, Purdue, Valentine, Storrs, in this country, are names which will long be honored for their liberal gifts in the interests of agriculture. They showed great sagacity, and not a little originality, by placing endowments in a new field, where gifts are few, and the opportunities for good are boundless. It is hoped that these illustrious examples will stimulate others to make similar bequests.

Where agriculture thrives, there we always find a prosperous people. She needs more trained minds to work in her interests. With better thought would come great and needed improvements in the agricultural department of the nation. It lacks means, strength, and stability.

The matter of plans, and the naming of a competent director of the geological survey, were referred to the National Academy of Sciences, whose suggestions the government authorities sought and adopted.

The same body, or the Standing Committee of this Association, or the members of the Society for the Promotion of Agricultural Science, would be amply competent to name a good man for commissioner of agriculture. Such a plan would throw the position more out of politics, and it would be more likely to run smoothly on, like the work of the Smithsonian Institution.

Greater permanency would tend to make the department more efficient, and help it to coöperate with the agricultural departments of the several states and the agricultural colleges and experiment-stations.

The leading object of these remarks is to call the attention of those who are working for the advancement of pure science to the great needs of agriculture, the grand opportunities for making discoveries, and the lasting gratitude which such workers are likely to receive from the people. Of course, we grant that all science is valuable, that much of pure science has a practical bearing, that no one can foretell what practical results may be reached by

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investigations in pure science; still there is a tendency among scientific men to ignore economic science.

I will illustrate my meaning. The U. S. Signal Service is generally supposed to have been established in the interest of science, with the avowed intention, also, of benefiting navigation. The benefits in these respects are certainly worth all they cost, but these are not all the benefits which the service should recognize.

I note the following as given by Dr. R. C. Kedzie some months ago, to illustrate the tardiness of science and the government to grant assistance promptly to the interests of agriculture.

"No industry, except navigation, is so completely at the mercy of the weather as agriculture, in its widest sense. In the magnitude of the interests thus threatened, agriculture outweighs all others in importance. Indeed, without the sustaining influence of agriculture, commerce itself would vanish like the dew of morning. Timely warnings of impending meteorological dangers might be given by the signal-service, which would be of incalculable worth to agriculture."

He illustrates the subject by referring to the protracted rainy weather during the wheat-harvest of 1882, in Michigan, where the loss was very great. "The approach of a protracted storm was known for days before the damage was done. If specific warning had been given our farmers at that time, most of the wheat might have been safely housed, and the farmers of Michigan saved from a loss of more than $1,000,000. The damage inflicted in this way is not isolated and exceptional."

At length the growers of cotton and tobacco in the south, and of cranberries in New Jersey, have been recognized by the government, and warnings of approaching frosts have been promptly given. "The general government, through the signal-service, should hold the shield of its protection over land as over sea, over corn fields as over tobacco-plant, over hay-field as over cranberry-marsh, over wheat-field as over cotton-plantation, over orchards and vineyards, and the cattle upon a thousand hills and prairie leas. Why not extend this work into wider fields by doing for the *producer* what it has so well done for the *carrier?*"

The opinion seems to be too prevalent that few experiments in agriculture are worth attempting, unless it be those conducted by a chemist. This is by no means the case, though it is true that none but a chemist is capable of making those of a certain nature.

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A physicist will still find in the soil much to interest him, and there is, no doubt, a chance to make discoveries valuable to agriculture.

With regard to the great importance of investigations and united action concerning the control of various plagues of our domestic animals, we should suppose no one would give a dissenting voice. Some valuable investigations have been made concerning the cause and nature of these diseases, among the most interesting of which, it seems to me, are the experiments made by Dr. Salmon in using an attenuated virus for inoculating animals, and inoculating again and again with a stronger virus those not affected by the attenuated virus. If the subject of animal plagues and the means of controlling them were fully discussed at meetings of this association, it would tend to allay prejudice, enlighten the minds of our citizens, and stimulate our law-makers to action. That there is need of a more general knowledge of this subject, I quote from a recent article by Professor Law in the proceedings of the Society for the Promotion of Agricultural Science. "The present agitation on behalf of legislation for the extinction of this lung plague in America began actively in 1878, and, notwithstanding that the subject has been continually before federal and state legislators for four years, but little real progress has been made. Among the drawbacks that may be specially named is the ignorance of legislators, of executives, and even of electors, on this subject."

In learning how to feed domestic animals economically, there is a great opportunity for investigation. There is much of interest and value to be learned in reference to the causes of fluctuation in weight of animals which are carefully fed and watered in a uniform manner.

Concerning the great need of continued and increasing efforts to investigate our injurious and beneficial insects, I need say but little; for the subject has been kept before the people, and the people are always interested to know something about an insect as soon as it injures their crops, or causes them trouble in any way. There is especially much need of more experiments to find better remedies for injurious insects. Attention to this portion of the subject cannot fail to meet with some degree of success. Success here is sure to win the favor of every one engaged in agriculture. Success in finding good, cheap, and safe remedies for injurious in-

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sects will tend to make science popular, and make endowments for research much easier and more frequent than ever before.

I need hardly add, that he who finds or breeds a race of honey-bees which is hardier, more industrious, longer-lived, quieter, possessed of longer tongues, and, last but not least, possessed of blunter stings, with less inclination to use them,—he who can succeed in any or all of these objects is entitled to rank with the man who shall cause two blades of grass to grow where only one grew before.

The U. S. Commission on Fish and Fisheries is an example of good scientific work, with prospects of early returns in the form of an increase in knowledge and a large increase in the supply of fish. A somewhat similar work, conducted by Prof. S. A. Forbes of Illinois, is in progress, where the object of the survey is to inquire into the food of birds and the food of fishes.

Some valuable scientific work of an economic nature has been done in connection with the tenth census, conspicuous among which is that performed by Prof. C. S. Sargent, in the study of forestry.

Botanical explorers in every land have repeatedly and liberally contributed plants of economic importance to the horticulturist,—a few new fruits, but more especially flowers and foliage-plants. An occasional contribution has been made to agriculture in the form of plants which promised to be of value for seeds or forage, or for some other purpose.

I have often been surprised that more attempts had not been made to secure the introduction of some new foreign grasses, and test them to ascertain their value for meadows and pastures. To be sure, grasses from western Europe have been tried; but we need others.

More than twelve years ago this idea appeared in my address on grasses, as given before the Northwestern Dairymen's Association, where the advice was given to yet other grasses from Japan, China, central Asia, and the dryer portions of South America. The cereals and pasture-grasses, the world over, are of more value to man and his domestic animals than all other plants taken together; yet the list of pasture-grasses now generally sown in any state can be counted on the fingers of one hand. In Great Britain, where much attention has been given to the subject, twenty-five or thirty species are much cultivated. It is hard to give all the reasons why so few grasses are employed in this country, but the fact remains

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that few are cultivated. The grass family is a large one, containing from thirty-one hundred to four thousand or more species. They are widely distributed in nearly all parts of the habitable globe, in every soil, in society with others, and alone. This does not convey an adequate idea of their value in unwooded regions, because the number of individuals of several of them is exceedingly large.

I have recently found the following in the American Agriculturist for 1858, a statement probably made by Dr. Thurber. "A dozen sorts, probably, cover nineteen-twentieths of all the cultivated meadow-land from Maine to Texas. It can hardly be supposed that so limited a number meets, in the best manner possible, all the wants of so great a variety of soil and climate. This is one of the pressing wants of our agriculture. Experimental farms are needed where the value of new grasses and kindred questions can be determined. A single new grass, that would add but an extra yield of a hundred pounds to the acre, would add millions of dollars annually to the productive wealth of the nation."

Still farther back, in 1853, the late I. A. Lapham of Wisconsin expressed similar views; and still longer ago, it 1843, forty years ago, in a prize-essay, J. J. Thomas said, "The great deficiency in number and the variety of our cultivated grasses has been long felt by intelligent cultivators." In this subject but very slow progress has been made in forty years.

In the extensive unwooded regions west of the Mississippi, the native grasses afford much pasture; but many of them start very late in spring, and stop growing early in autumn. They do not completely occupy the ground: they are easily stamped out by the hoofs of cattle and sheep. Some of the tame grasses will thrive better, and afford much more pasture.

In Science, vol. i, p. 186, of this year, Prof. N. S. Shaler refers to this subject. He says: "It seems possible to improve this pasture by the introduction of other forage-plants indigenous to regions having something like the same climate. The regions likely to furnish plants calculated to flourish in a region of low rainfall include a large part of the earth's surface. Those that would succeed in Dakota are not likely to do well in Texas or Arizona. For the northern region, the uplands of northern Asia or Patagonia are the most promising fields of search; while for the middle and southern fields, the valley of the La Plata, southern Africa, Aus-

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tralia, and the Algerian district, may be looked to for suitable species." He recommends three experiment stations,—one in Nebraska, one in Texas, and one in Arizona.

In this connection, when we remember that exotic plants often thrive better than natives, we see what a vast field lies ready for experimenting with the grasses.

Grasses look much alike to all who have not closely studied them; so that farmers, in fact, none except botanists, are likely to attempt experiments. This is a strong reason why the state and national governments should assist agriculture in an undertaking which seems so fruitful of good results within a short time, at so trifling an expense. Expeditions are sent at great expense to explore polar seas, with a view to slightly extending our knowledge of a barren portion of the earth's surface. Large sums are employed to fit up in magnificent style, and to send to the remotest parts of the earth, expeditions to spend a few minutes in observing an eclipse or a transit of Venus. Would the sending of competent persons around the earth in search of better grasses be an undertaking less praiseworthy?

The men who control the Northern Pacific Railway were enterprising enough to see that a complete economic survey of the adjacent territory would help the sale of their land. Among other things, the grasses will be carefully examined.

For the past ten years the writer has been testing, in a small way, some hundred and fifty species of grasses. These, with few exceptions, are natives of the eastern United States and western Europe. I am fully convinced that further experiments, carefully made on a larger scale in several portions of our country, would be quite sure to result in great gain to agriculture.

Grasses suitable for the western prairies, to take the place of those which will be rapidly stamped out by close feeding, are sure to be found even without the aid of the government; but greater time will be required.

Prof. E. M. Shelton of Kansas Agricultural College has probably done more than any one else in the west to test grasses and clovers, and diffuse information in regard to the results, which are most gratifying. At nearly all the gatherings of farmers in the west, this question of new grasses is a prominent topic of discussion.

Wherever irrigation has been well tried, especially on land which

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is light and well drained, the results have been quite surprising, converting a dry, hungry meadow into a little oasis. Such a meadow is the triumph of agricultural art.

One of the most remarkable results of irrigation, as viewed by a scientific man, is this: the list of grasses will not remain the same, or maintain the same proportion. The bad grasses will nearly all die out, or improve in quality; while the best ones will rapidly increase. And again: experiments in England have shown that irrigation causes many herbaceous plants, distinct from grasses, such as plantain and buttercups, to give place to good grasses. Docks are not diminished by irrigation. The best grasses are a sign of good land in fine condition. Such grasses are hearty feeders, and are most sensitive to good treatment. In a well-managed meadow, irrigation in four years increased the value threefold.

Solon Robinson long ago expressed the view, that, if the streams of the Connecticut were properly utilized in irrigating the soil, they would be more productive in value than by turning all the water-wheels of the state. More experiments in irrigation are much needed in this country.

Baron J. B. Lawes, a most renowned experimenter in agriculture, possessed an old pasture which had been in permanent grass over a century. No fresh seed of any kind was sown during this period. For some seven years or more, he experimented by applying to this old pasture, on different plats, twelve different kinds of manures. The results were very interesting and gratifying. "The manures, which much increased the produce of hay, at the same time very much increased its proportion of graminaceous herbage. The total miscellaneous herbage (chiefly weeds) was the most numerous in kind, and nearly in the greatest proportion, on the *unmanured* land,—viz., sixteen per cent,—while on the manured plat it decreased to two per cent. Every description of manure diminished the number of species and the frequency of occurrence of the miscellaneous or weedy herbage. A few weeds were increased by the manures, such as Rumex and Achillæa."

"The plants of a meadow," in the words of the Agricultural Gazette, "live in harmony on the unmanured open park, having nothing to fight for in a state of nature; but toss them a bone, ground fine, or any other choice bit, and their harmonious companionship terminates at once. Every act of improved cultivation occasions instant war. A grass likes the best that can be got.

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It will swallow soda, but not when it can get potash. As a general principle, all manures tend to drive out the weeds by increasing the better herbage." A repetition of like experiments in this country could not fail to give valuable results.

In Europe some success has been reached in selecting and cultivating different varieties of *Lolium perenne*, *Dactylis glomerata*, and *Trifolium pratense*.

The field is a promising one for any careful and enthusiastic student. For three years past, I have been studying hundreds of plants of red clover at all seasons and stages of growth. I have plants growing, the seeds of which came from marked plants which varied much from each other. Plants in the fields of red clover vary amazingly in many respects, which influences their value for forage-crops. I believe our fields of red clover to-day contain nearly or quite as great a variety of plants as would a field of Indian corn, if we were to mix in a little seed of all the varieties cultivated in any one state.

Some of our grasses in cultivation are quite variable, notably the fescues, orchard-grass, and perennial rye-grass. It was some time ago observed that alfalfa of California, and lucerne of Europe, were quite different in their capacity to endure dry weather, though they belong to the same species. Different treatment in widely separated countries for many years has wrought a great change.

The subject of changing seed, planting old seed, mixing seed, raising it one year or more in a remote country and then returning to the starting-point, deserves the attention of careful experimenters.

The late Charles Darwin experimented on the effects of cross and self-fertilization of plants, and found that in most cases plants from crossed stock were earlier, hardier, germinated better, and yielded more seeds, than those from seed of self-fertilized plants, while crossing with foreign stock of the same variety is a far greater improvement. The idea is to cross the flowers of a plant with pollen from other plants of the same variety, the seeds of which were raised pure for five or more years in a remote locality, fifty miles or more away.

Mr. Darwin said, "It is a common practice with horticulturists to obtain seeds from another place, having a very different soil, so as to avoid raising plants for a long succession of generations under the same conditions; but, with all the species which freely intercross by the aid of insects or the wind, it would be an incomparably

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better plan to obtain seeds of the required variety, which had been raised for some generations under as different conditions as possible, and sow them in alternate rows with seeds matured in the old garden. The two stocks would then intercross, with a thorough blending of their whole organizations, and with no loss of purity to the variety; and this would yield far more favorable results than a mere exchange of seeds."

In a word, with plants which may be easily crossed, get some foreign seed of the same sort and mix with your own seeds to raise seeds for ensuing crops.

In 1877 I began some experiments of this kind with Indian corn and with beans, and have since made others. The advantages shown by crossing of corn over that not crossed was as 151 exceeds 100, and in the case of black wax-beans it was as 236 exceeds 100. Since then similar experiments have several times resulted in showing a large increase in favor of crossing with foreign stock.

In a review of Darwin's book, the Gardener's Chronicle of England said in 1877, "It is certain that these practical results will be a long time filtering into the minds of those who will eventually profit most by them." The results of my experiments have been widely printed in the agricultural papers of the day, and have been given at numerous farmers' institutes and granges, beginning in the winter of 1877, nearly six years ago; and yet I cannot learn that any other person in this country has attempted similar experiments. I will make one exception, in the case of Prof. W. A. Henry of Wisconsin University, who tried the experiment in connection with myself. The results, so far, fully accord with the prophetic statement above quoted from the Gardener's Chronicle.

In originating new varieties and races, see what has already been done, largely in our own country, in a haphazard way, with strawberries, raspberries, blackberries, gooseberries, and grapes, to say nothing of improvements in ornamental plants.

I need hardly add, that some of the best results, considering the means and time employed, have been obtained by persons who have crossed and hybridized according to some well-devised plan.

Our varieties of fruits in cultivation have become so numerous, that to describe them by the fruit and foliage alone often baffles the skill of the most expert pomologist. In the proceedings of the American Pomological Society for 1877, 1879, and 1881, I have shown that much help can be obtained by noticing the peculiarities

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of the flowers of apples and pears. The same is no doubt true, to some extent, with grapes, peaches, gooseberries, and other fruits.

Here is a promising field, full of interest to the botanist, — a field where he may accomplish much to aid the horticulturist, and something to advance science. A new variety of any cultivated fruit can no longer be considered as well described, unless some account be made of the flowers.

It has often been shown that many kinds of insects are beneficial to plants by aiding the fertilization of the flowers. The subject has still about it much that is new. Even Mr. Darwin said he did not suppose that he fully understood all the contrivances for fertilization in any one flower.

If it may be true, as my experience during the past six years help to indicate, that bumble-bees aid in fertilizing red clover, then farmers should try and encourage these interesting insects, even though they be disagreeable companions. Bumble-bees prefer to raise their colonies in old nests of meadow mice. I mentioned in my last report, that it had been suggested that we should not keep many cats, nor allow hawks, foxes, or dogs to catch these mice; for they make nests which are quite necessary for the bumble-bees, which help to fertilize our red clover, and thereby largely increase the yield of seed.

Perhaps it may not be altogether visionary to predict that men will yet engage in raising bumble-bee queens, and sell them to farmers at a fair profit, for starting colonies to improve the yield of clover-seed. We may yet have conventions and societies where the leading object shall be to discuss the merits of different sorts of bumble-bees.

A few years ago experiment-stations in Europe began testing seeds which were offered for sale in the markets. Adulterations were discovered most ingenious in character, harmful in effect, and remarkable in amount.

The more the subject was investigated, the worse it seemed to be. Something of the same sort has been undertaken in this country, showing that even in Michigan some worthless seeds are put on the market. In 1877 and later I tested large numbers of vegetable-seeds purchased of fifteen of our large dealers and growers. Not one of these is free from selling seeds that are worthless. The remedy is not easy. On account of its effect on their advertising, publishers are unwilling to print for their readers the results

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of these experiments. Only a few people can acquire the information after experiments are made.

In making tests of seeds, we still lack information in regard to the surest and best mode of testing each sort. Here is a good work for some accurate and ingenious scientist to invent new apparatus, learn the proper amount of heat, air, and moisture, for producing the best results, find out whether seeds will thrive best with a constant temperature, or a variable temperature; and learn the best modes of preserving seeds alive from one year to another.

I need hardly mention to intelligent students, that there is an extensive field, a very attractive one, in the study of fungi. The agriculturist, who deals with plants, not only wants to know the kinds, but the requirements which are favorable or unfavorable to their development. In the study of effectual remedies against fungi, something has been done; but there is still much demand for more knowledge. Successful experiments in regard to fungi are not likely to be made except by botanists.

I have only glanced at a few points where the biologist can find interesting work which will give threefold returns by advancing science, helping to elevate agriculture, and benefiting our country. There are many experiment-stations in Europe, and some in this country. We hope their number may soon increase, and that liberal and permanent endowments will not be lacking. This Association, and all other societies working in the interest of science, can render a great service by doing what they can to encourage experiments in all departments of agriculture. Men can be encouraged to prepare papers, and committees can make reports pertaining to the subject. There is a need of thorough state surveys, solely with a view to the interests of agriculture and kindred subjects. More knowledge of our soils, water, building-materials, plants, timber, injurious fungi, insects, and birds, would return to a state fivefold the cost of acquiring such information. In brief, then, as one of the humble workers in the interests of agriculture, I most cordially invite you to turn your attention to some of the problems which vex the husbandman.

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PAPERS READ.

AGRICULTURAL BOTANY. By E. LEWIS STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

IF kitchen garden plants be closely studied in many varieties, it will be found that selection has differentiated the various natural species in accordance with desired uses.

It will be noticed that while there is a striking uniformity within varieties in those portions of the plant which have not been selected for improvement, there is a great variation between those portions which have secured attention on account of their uses. Thus, in forty-five varieties of onions growing side by side, the foliage is all similar, yet the bulbs vary in size, color, shape and habit of formation. The twenty-two varieties of carrots present like foliage, yet unlike roots. In sixty kinds of lettuce a likeness of bloom and great unlikeness between plants while of edible size. Among sixty-six kinds of tomato a sameness of bloom, certain variations in foliage and growth habit, and a very marked diversity in form of fruit borne.

It follows from a careful observation of over eleven hundred named varieties of kitchen garden plants, that two series of variations can be distinguished: the least marked, the normal variations that occur between the parts not subjected to conscious selection as being of little account for use; the very marked, or artificial variations that are produced by conscious selection exerted upon the parts which are valued for their use, and which selection has been exercised most stringently upon form.

It will be further noticed that according as selection for the same purpose has been exercised upon the various parts a parallelism of development has taken place between plants of different species, genera and orders. This parallelism of development points to a unity of arrangement in the forces by which the correspondences are produced.

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The effect of selection concentrated upon visible forms has been to produce and fix changes from the natural plant to such an extent as in cases to mask the original species so that historical data must supplement morphological data in order to connect the genetic record. Thus, for illustration, there at first seems no specific connection between certain cultivated kales, cabbages and cauliflowers, while a specific connection of these with the ruta-baga seems uncertain. It is only as these forms are produced through the sowing of the seeds of *Brassica oleracea* that their close relation becomes manifested.

In the case of some long cultivated plants the original prototype is unknown, and can be sought but by conjecture. Such is the case with many of our cereals, with lettuce, etc.

In some plants selection has been exerted in different directions, as, for illustration, with the beet, whereby the root beet and the leaf beet have been produced; the celery and the celeriac; the parsley and the Hamburg parsley; the onion and the top-onion, etc.

It is clearly evident that conscious selection is a powerful agency for the changing of form, and by long exercise can overcome the norm or type affixed by nature to a species. The direction of this change and its consequences are determined by man in the direction toward usefulness to him as distinct from nature's intent toward the maintenance of the species.

We hence have a different set of motives governing the domesticated plant than those which govern the feral plant.

The individuals of a natural species cluster about a common type, held in place by a natural environment reacting with the force transmitted by heredity. The individuals of an artificial species (domesticated variety) cluster about a common type formed and continued through man's agency whereby an assisted environment reacts upon heredity modified by selection in influencing plant form.

In the feral plant genetic resemblance, as observed or inferred, furnishes a method of classification in accord with the law of evolution as expressing harmonies of action of special causes. Through the reaction of natural heredity with natural environment, classes are already formed for us and but await our discovery.

In the domesticated plant the power of intelligence to eliminate, modify and direct the action of natural laws under a given purpose,

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introduces a new factor to influence plant growth, and forms designed for uses mask genetic resemblances in those portions of the plant where change means value to man. In accordance with the law of evolution, but under a new application, form becomes paramount as a motive over genesis: usefulness, freed from the motive of self-maintenance, becomes paramount over the motive of continuance of the species. Through the reaction of an assisted heredity with an artificial environment including conscious selection, in artificial plants, classes are also already formed for us, and but await our discovery.

If, in nature, classes are formed through considering genetic data as paramount, then the natural system is in force for classification. If, under art, classes are formed through human selection based upon form as paramount, then an artificial system of classification is as truly scientific, and expresses in like manner as true a set of relations, but relations of a different character, as does the natural system. In a scientific artificial arrangement we seize upon a quality or qualities which yield us information as to the motive or development for uses, and which furnish signs of natural laws as modified and influenced by man, and form of selected parts furnishes the key; in a scientific natural arrangement we seize upon a quality or qualities which, through equal correlations yield signs of natural laws, and the floral organs, as indicative of the motive of the wild plant's existence, furnish the key.

If these views are correctly stated, then it is seen that an Agricultural Botany as an annex to the Natural Botany is imperatively required for the purpose of furthering classification and indentification of domesticated plants, and such an annex must vary in its methods as widely from the methods of the natural botany as cultivated plants vary from feral plants, the key to the motive being in one case the form of parts in use, in the other the floral organs.

INFLUENCE OF POSITION ON SEED. By E. LEWIS STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

IN trials carried forward at the New York Agricultural Experiment Station last winter it was found that for an average, 91 per cent of but kernels, 88 per cent of central kernels and 98 per cent of tip kernels of flint corn germinated; while for dent corns 70

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per cent of but kernels, 56 per cent of the centres and 60 per cent of the tip kernels germinated. Our trials were, however, all too few in number to justify generalization without further evidence in the field. The following table represents the result of planting 3420 each of but, central and tip kernels, the three kinds taken from the same ears, and representing the five but, five central and five tip kernels of each row of the Waushakum Flint corn.

There were five plats, each containing three rows of each kind of seed: 38 hills with 6 kernels in each, or 228 kernels to a row, or 684 kernels of a kind in a plat. Planted May 17, vegetated May 28, final count June 8.

VEGETATIONS.

Plat.	Buts.	Centres.	Tips.
I. A. 1	542	598	607
I. A. 2.	551	581	599
I. A. 3.	571	580	588
I. A. 4.	534	574	606
I. A. 5.	517	556	565
Total.	2715	2889	2965
Per cent.	79	84	86

Comparing these figures with the average obtained in other plats with ordinary seed of the same corn, as carefully selected.

Plat.								
E. 6.	2376	kernels	planted;	2019	vegetated,	or	85	per cent.
E. 7.	1188	"	" ;	1006	"	"	84	"
D. 1.	1126	"	" ;	790	"	"	70	"
D. 2.	1126	"	" ;	776	"	"	69	"

In a field trial on a smaller scale in 1882, 70 per cent of the but, 96 per cent of the central and 97½ per cent of the tip kernels vegetated. For the crop, calculating the merchantable ears to the 100 plants, we had,

	Merchantable ears yielded per 100 plants,		
	From but seed.	From central seed.	From tip seed.
Unmanured plat.	110	73	120
Manured plat.	112	107	117

	Unmerchantable ears yielded per 100 plants.		
	From but seed.	From central seed.	From tip seed.
Unmanured plat.	57	26	20
Manured plat.	27	14	12

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Av. length of merchantable ears. Inches.	From but seed.	From central seed.	From tip seed.
Unmanured plat.	7.57	5.55	8.13
Manured plat.	6.64	7.10	7.55

Preliminary trials towards establishing the limits within which our figures will apply, justify the conclusion that these figures offer, viz:—that for flint corn the tip kernels have the stronger vegetative power. It is part of the station work to establish the range within which variation may be expected to occur in agricultural work, or, in other words, to define the limits of probable error in various series of equivalent events, but our results are as yet not sufficiently advanced to offer to the public.

Parallelism of Structure of Maize and Sorghum kernels.

By E. Lewis Sturtevant of Geneva, N. Y.

[ABSTRACT.]

If kernels of flint, pop, sweet and Tuscarora maize be split parallel to the germ, each race will be seen to present a definite arrangement of structure. Thus, the flint corn exhibits a germ surrounded by starchy matter, and this in turn by a corneous envelope; in the pop corn proper the germ is enclosed in the corneous matter, the starchy matter being absent, except as the pop variety entrenches upon the flints. The sweet corn has a similar structure to the pop, but the corneous matter is translucent and wrinkled; in the dents the corneous matter occupies the sides, the starchy matter extending through to the summit, and including the germ; in the Tuscarora the corneous matter is entirely absent, and only the germ and the starchy matter are to be made out.

If kernels of sorghum be split in like manner, a similar arrangement is to be found. Thus, the seed of the varieties distributed under the name of "Dhourra," "African Wheat," and "Millo Maize" correspond to the flint corn type, the germ surrounded by starchy matter, and this in turn included in a corneous envelope. In "Rice Wheat" we find the pop corn type, germ and corneous matter, no starchy matter being visible. In "Neeazana" we have a structure modelled upon the dent corns, and in "Chinese Sugar Cane," a

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structure modelled after the Tuscarora, or starchy matter and germ without visible corneous matter.

On account of the confusion of vernacular names it seems unnecessary to introduce further examples, although an examination of some thirty or forty so-called varieties shows that this observation is founded upon general characters.

In the case of the maize kernel we might assume that these differences have been produced by consciuos selection, the kernel being the portion used, and the various races being fitted for uses founded upon the structure. In the case of the sorghum, however, the supposition would scarcely answer, as the kernel is of small size, and apparently has not been subjected to conscious selection. In view of this parallelism of structure, together with the probability that a like selection has not been exercised upon both species, it seems safe to infer that these structural differences within each genus have more meaning than simple variety differences, and that the same causes have been at work, independent of man's conscious selection, to produce the races which in Zea and Sorghum have such well-marked and allied characteristics.

Relation of Root and Leaf areas in Corn. By D. P. Penhallow of Montreal, Canada.

[ABSTRACT.]

The exact determination of the relation between the aërial and subterranean portions of a plant is a question which has an important practical bearing, but the obvious difficulties in the way of its solution have apparently deterred observers from any special effort in this direction, and in my own case have resulted in rendering many attempts fruitless. Measurements made during the summer of 1882, however, have given us figures which may be considered reasonably accurate; they are therefore given here, not as fully conclusive, but as indicative and for the purpose of enlisting the efforts of others in a similar direction, for a law which can be generally applicable in this case will only be secured after repeated trial and estimation. The plants selected for the purpose, were those of the white flint corn, four weeks from the ground. They were obtained without breakage of the roots by digging a

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trench parallel with the row and washing out by means of a hose. The plants were then taken to the office and the roots carefully separated from each other under a strong pump stream. The measurements of roots and leaves of the same plants gave the following results.

STEM.			
	Average circumference,	3.05	C. M.
	Length,	18.00	C. M.
	Total area,	54.90	S. C. M.
LEAVES.			
	Whole number,	8	
	Total area of one surface,	381.07	S. C. M.
	Total area of both surfaces,	762.14	S. C. M.
	Total area of transpiration,	817.04	S. C. M.
ROOTS.			
	Primary root, total length,	22	C. M.
	Laterals of primary,		
	With branches,		
	Whole number,	78	
	Total length,	510.30	C. M.
	Average length,	6.54	C. M.
	Branches.		
	Whole number,	1339	
	Total length,	2075.45	C. M.
	Average length,	1.55	C. M.
	Without branches.		
	Whole number,	207	
	Total length,	353.90	C. M.
	Average length,	1.71	C. M.
	Secondary roots.		
	With laterals.		
	Total length,	247.00	C. M.
	Laterals.		
	Without branches.		
	Whole number,	1086	
	Total length,	3138.84	C. M.
	Average length,	2.89	C. M.
	With branches.		
	Whole number,	18	
	Total length,	95.63	C. M.
	Average length,	3.6	C. M.
	Branches.		
	Whole number,	131	
	Total length,	35.63	C. M.
	Average length,	0.73	C. M.
	Without laterals.		
	Whole length,	74.80	C. M.
TOTAL LENGTH OF ROOT,		65.823 M. or 217.702 feet.	

DIAMETERS.			
A	Primary roots.		
	Secondary roots.	0.123	C. M.
B	Laterals of primary root,	0.048	C. M.
C	Branches of primary laterals.		
	Branches of secondary laterals.		
	Laterals of secondaries,	0.036	C. M.

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CIRCUMFERENCES.	
A	0.3864 C. M.
B	0.1507 C. M.
C	0.1130 C. M.
AREAS.	
A	132.844 C. M.
B	130.234 S. C. M.
C	604.047 S. C. M.
TOTAL ROOT AREA.	867.125 S. C. M.

In making these determinations, each root and fragment was measured, so that the entire length passed under inspection. The measurements were all made under a dissecting glass. The leaves were measured by dividing them up into a number of trapezoids and measuring each separately. By these precautions it is believed the unavoidable error was reduced to a minimum. It is proper to mention here that this work was rendered possible only through the hearty coöperation of two devoted students, Miss Chapman and Miss Hatch. The error remaining in the figures as presented, arises from inaccuracy in leaf measurement, and though inconspicuous, would have a tendency to make the results a trifle too small. The error in estimation of the roots would be more conspicuous, but would make the results too large. In view of these facts, it seems highly probable that the root and leaf areas normally approach a strict equality. If such be the case, then we might expect to find the root hairs, or organs of absorption, equalling in functional power, the stomata of the leaf or transpiratory organs. Numerous attempts to determine the number of root hairs upon a given area have always met with great difficulties. As new hairs are constantly being formed and old ones as rapidly passing away, or, what is worse for our purpose, becoming dead but still persistent, it is quite impossible to determine just where to draw the line of functional activity and select only those which are strictly equivalent to the action of the stomata in the leaf.

The results obtained, however, from a large number of averages, showed a total of 10561.00 per S. C. M.

As a result of estimation by three separate observers, the stomata were found to be for the

Upper surface of the leaf,	5411 per S. C. M.
Lower surface,	6980 per S. C. M.

These figures were obtained from a very large number of averages, and may be considered reasonably correct, since the extreme

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percentage error for the three observers was not more than two and one-half.

Comparing the whole number of stomata for both surfaces of the leaf, 6195.5, with the whole number of root hairs, 10561.00 per square centimetre, a very considerable discrepancy will be found in favor of the latter. It should be borne in mind, however, that while it is comparatively easy to determine the proportional part of a stoma which occupies a given square of the micrometre, it is not so easy to determine exactly the full number of root hairs in the same area, and thus serious error is sure to arise. Again, the determinations were made upon a basis of 1-10000 of a square centimetre, hence all errors are multiplied just 10000 times. Therefore, if we go back to the original area of estimation, we shall have for the stomata 0.61955, and for the root hairs 1.0561 per 1-10000 of a square centimetre. While this number may represent an approximation to the true ratio, we cannot assert with any assurance that the relation is at all well defined so far as mere numbers are concerned. The size of a stoma is subject to little or no variation after it reaches the period of functional activity, and its power to permit the transpiration of a given quantity of water is controlled only by the continuation of its vitality and not by its stage of growth. In the root hairs we have organs which are performing their functions during the entire period of an active growth. New hairs are constantly arising where old ones are becoming useless, and thus with such diverse lengths, different areas may represent entirely different quantities of functional energy. Moreover, the number of root hairs to a given area is largely controlled and modified by the special conditions of growth, while the same is not true, in the same degree at least, of the transpiratory organs. Thus in time of drought, the roots develop an excess of root hairs to supply the demands of the leaves which remain more nearly constant. At such times, however, we do not find an increased development of leaf and stomata, for the plant already has too many. On the other hand, there is a strong check upon their further development and thus from a two-fold direction, there comes in a strong element in favor of a disproportion between the special organs of transpiration and those of absorption. Again, from the fact that the water taken up by the roots is partly used up in the chemical processes of the plant, while the remainder is transpired, it follows that the root hairs

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must either exceed the stomata in numbers or in size and functional energy.

All this taken into consideration would lead us to believe that, while under wholly normal conditions of growth, there may be a more or less definite numerical relation between root hairs and stomata of the leaves, under ordinary conditions of growth this relation is more strictly dependent upon functional considerations alone, and that it cannot be exactly expressed by numbers. Whether we recognize the root hairs as strictly the functional equivalents of the stomata or not, it is true that we must admit the functional equality of the roots and leaves as a whole. An extension of this thought would lead us to see that these organs are equivalents in the first place as mere areas of extension; in the second place their equality functionally depends upon (*a*) their degree of specialization and (*b*) the necessity for specialization of certain parts. As a matter of fact, if the roots absorb at all as such, this power is confined to a short distance near the tip where growth is active but it ceases as soon as the epidermis is replaced by a corky layer, and this occurs very soon.

If the leaves, as such, perform the functions of transpiration, their power in this direction is very largely controlled by the special conditions of growth as modifying their structure, since we know that, while under ordinary circumstances, the epidermal structure is so developed as to permit a certain amount of transpiration, in many cases, as in cacti, there is a specialization of this structure for the express purpose of retarding loss of moisture. We must consider, therefore, that, primarily organs of extension, the functional value of both roots and leaves must depend chiefly upon the degree in which certain parts are specialized, and this we must look for in the hairs and stomata. As we have seen, moreover, that roots in general are incapable of absorbing moisture beyond a very limited extent, while leaves and all green parts of the plant not too highly cuticularized may partake of the transpiratory function, we may reasonably assume that part of the inequality shown in our figures may be accounted for, and is compensated for in this way.

In the absence of confirmatory observations, we must assume for the present, that these indications are correct; that nature maintains a constant and well defined relation between the portions of the plant exposed to the air and that covered by the soil.

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Careful reflection cannot fail to show the disturbance of normal functions which must invariably follow any act upon the part of man, by which this relation is disturbed. Applications of the principle must readily suggest themselves to the intelligent reader in such operations as transplanting and similar processes where a portion of the root is likely to be removed.

ORIGIN OF THE FLORA OF THE CENTRAL NEW YORK LAKE REGION. By WILLIAM R. DUDLEY of Ithaca, N. Y.

AN ABNORMAL ORCHID. By WILLIAM R. DUDLEY of Ithaca, N. Y.

[ABSTRACT.]

Two years since the writer found some thirty specimens of *Habenaria hyperborea* growing in dry ground under beech trees, all the flowers of which were destitute of spurs; and in all cases the labellum was so changed in form as to resemble the labellum of *Habenaria dilatata* the nearest congener of *H. hyperborea*. It will be seen from the drawings accompanying this paper as read, and from the specimens themselves, that it is not a case of ordinary "peloria."

The condition of peloria is excellently illustrated by drawings of an abnormal *H. tridentata* which accompany this paper, as read.

This case of *H. hyperborea* comes under no one recognized class of teratological variations, but is evidently caused by an arrest of development; and if we may suppose the general law to hold that variations produced by this cause usually show a more primitive form of the species, then this variation of the lip or labellum toward *H. dilatata* indicates the latter species to be the older species or a more primitive type, while *H. hyperborea* may have originated from it. The absence of the spur would seem to point to the conclusion that it was one of the latest modifications of the Habenaria flowers, the one most easily lost in any unusual strain upon the plant's resources.

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Note on the Present Condition of the Box Huckleberry, Vaccinium Brachycerum Mchx., in Perry County, Pa. By E. W. Claypole of Akron, Ohio.

[ABSTRACT.]

Michaux described a species of Huckleberry from Virginia under the name of *Vaccinium brachycerum*. This plant cannot now be found in either of the two habitats mentioned by that author. It is possibly extinct. Its chief habitat now is in Perry County, Pa.

The plant occurs on a hill side composed of the Chemung shale near New Bloomfield. It is very abundant on this spot but its range is very small and remarkably restricted. It does not cover more than about ten acres of ground in a piece of woodland, but the causes which determine its limitation are not easily seen. In one direction it grows down to a small run forming a perfect mat to the water's edge and on the other side only two yards away not a plant can be seen. In the wood also it is possible to walk with one foot buried in a mass of this plant and the other where not a specimen is growing. The line of its growth is marked almost as plainly as if by an invisible fence which it does not pass.

This pretty little huckleberry is hovering as it were on the very verge of extinction. The clearing of the few acres on which it grows and ploughing for one or two seasons would totally eradicate it and I know of no other place in the county where it occurs. I have heard that it is to be found on a neighboring hill but have not found it.

The fruit which is now ripe is edible but possesses less flavor than the other huckleberries. In appearance it most resembles the low blueberry being covered with a bloom. It is worthy of notice that it does not fruit freely. This year when the crop of other huckleberries is unusually large the berries of this species are scattered singly and sparingly over the plants. Possibly we have here one cause of its diminution. It flowered very abundantly.

The plant grows from six to ten inches high, is evergreen, but turns red in the winter.

The only other locality for it known to me is on the bank of Indian River about tidewater, in Newcastle Co., Delaware, where it has been found by Mr. Commons, who has kindly sent me specimens exactly resembling those of Perry Co.

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A Supposed Poisonous Seaweed in the Lakes of Minnesota.

By J. C. Arthur of Charles City, Iowa.

[ABSTRACT.]

This paper gave the incidents which led to the apprehension of a certain alga of Minnesota lakes as the cause of considerable mortality among domestic animals. In July of 1882 several cows and young cattle, and a small number of horses and hogs died at Waterville, a small place about sixty-five miles south of Minneapolis, in such a manner that the probable cause was traced to the water they drank, which in every instance was that of the lakes in the vicinity. These lakes are of various sizes, Lake Tetonka, the largest, covering a little over two square miles, and are in no way peculiar except in being connected and drained by a small sluggish river. The water of the lakes and river at the time of the trouble was green with a small floating alga, which pervaded the water and formed a scum on the surface that the wind collected into masses along the shores. The alga proved to be the same as one described by Dr. Cohn of Germany under the name of *Rivularia fluitans*. The synonymy of the species has not yet been traced, and it may have been described under an earlier name. The Waterville plant, also detected during the present session in Lake Minnetonka, consists of little balls of the uniform size of mustard seed, of a dark olive-green color, turning reddish-brown when dead, which, contrary to the usual habit of members of the genus *Rivularia*, float free. The minute anatomy has been given elsewhere, and it is rather the design of this paper to call attention to the plant's supposed poisonous qualities. In both 1882 and the present year, by the time the spot could be reached after notification that the plants had attained their maximum abundance, the danger was passed, through the almost total disappearance of the alga, and in consequence no experiments could be undertaken. The history of the circumstances, elsewhere given in detail, shows that while the evidence of poisonous qualities in this plant is purely presumptive, it is seemingly well founded. The observations thus far have been under the direction of the Department of Agriculture of the University of Minnesota, and are to be continued.

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Relations of certain forms of Algæ to disagreeable Tastes and Odors. By W. G. Farlow of Cambridge, Mass.

[ABSTRACT.]

Although large masses of any decaying vegetation may render water unfit for drinking, the only group of plants to be feared, as far as their effect on the taste and odor is concerned, is the members of the nostoc family, which form floating scums of a bluish-green color. When exposed to a bright sun, especially in shallow water, they are transformed into fœtid, repulsive-looking masses of slime, which give to the water the so-called pig-pen odor. The water-supplies of several eastern cities have been thus contaminated, and principally by species of Cœlosphærium, Clathrocystis, and Anabæna. In Minnesota is the representative of a fourth genus, Rivularia, which was first found last year at Waterville by Professor Arthur, and which has been found to be very abundant this year in Lake Minnetonka; and in all probability it occurs in most of the other lakes of this region. The singular fact is, that while unknown elsewhere in this country, the species was found several years ago by Cohn in Silesia, who named it *Rivularia fluitans;* and it was detected also by Gobi near the Gulf of Riga. It appears also to be very closely related to, if not identical with, an alga abundant in certain parts of England, referred by Harvey and more recently by Philips to *Echinella articulata* Ag. This is another illustration of the very wide distribution of the species of the nostoc family, of which we have other recent illustrations in the *Nostochopsis lobatus* of Wood, first described from the northern states, but which has since been found to be identical with *Mazea rivularioides* subsequently discovered in Brazil; and again in *Hormactis Quoyi* which is found only at Falmouth, Mass., and the Marianne islands in the Pacific.

There is a strong probability that, in the future, Minneapolis may be troubled by the decay of the different nostocs floating in the lakes near the city, where they are very abundant. As far as avoiding trouble from these plants is concerned, undoubtedly river-water is to be preferred to lake-water; but before many years the Mississippi near Minneapolis will be contaminated by sewage, and the water will probably then be obtained from the lakes. If the shallower lakes near the city are used, there can be little doubt that in

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summer Minneapolis will have the same trouble as that experienced in Boston. Even at greater expense, the water should be brought from large and deep lakes, especially those across which the winds sweep so as to keep the surface-water roughened.

THE SPREAD OF EPIDEMIC DISEASES IN PLANTS. By W. G. FARLOW of Cambridge, Mass.

[ABSTRACT.]

IN the case of animals it can be said, that, excepting the diseases attributed to bacteria, they are subject to but few diseases caused by fungi. In the case of plants, however, the greater part of the diseases to which they are subject are caused by parasitic fungi; excepting, of course, the injuries caused by insects, which need hardly be considered in speaking of epidemic diseases. Most of the violent epidemic diseases of plants are caused by fungi of the orders Uredineæ, rusts, and Peronosporeæ, rots. Fortunately, the species of these orders attack only a single species of host, or at most several species closely related botanically; so that, for instance, a rot which would attack the potato would not probably attack the grape, although it might be expected to attack the tomato, which is botanically closely allied to the potato. As might be expected, the most violent epidemics occur during, or just after, unusually wet periods. An epidemic disease spreads either by the dispersion of its spores through the air, or by the transportation of the host-plant on which it is growing; the latter being probably the means by which diseases are carried across large bodies of water, as the Atlantic.

With the introduction of food-plants from Europe to this country come, of course, many of their parasitic diseases. It should be noted, however, that the most violent plant-epidemics of recent times have advanced not from east to west, but from west to east. The best known case is that of the potato rot in 1845, and since then the very accurately recorded case of the grape-mildew, *Peronospora viticola*, has arisen. In the first case, the disease is supposed to have reached Europe from the west coast of South America, by way of the United States. In the latter case, the grape-mould,

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which is a native of North America, can, as I showed by experiments in 1876, be transferred to the European vine; and it was prophesied that the disease would extend to Europe and do more harm than with us. The prophecy was very soon fulfilled, as you all know. In the two diseases just mentioned, it is a characteristic of the spores that, in germinating, instead of giving off a filament, they discharge a number of motile zoöspores, each of which is capable of propagating the disease. We have several other species of Peronospora, which produce zoöspores, some of which have apparently crossed from America to Europe; and there are others which, although common in this country, have not yet appeared in Europe, although, following the grape-mould, they may be expected to appear there hereafter. Among these may be mentioned *Peronospora Halstedii*, which grows on composites, and may later be found in Europe on the Jerusalem artichoke. Professor Trelease has recently found a Peronospora on Sicyos in Wisconsin, which resembles the grape-mould in general appearance. The germination of the spores has not yet been observed, but judging by analogy one would expect them to produce zoöspores. It would not be surprising if the Peronospora on Sicyos should also be found hereafter causing a disease of squashes or melons; and its progress eastward might be expected as in the cases previously cited.

Reference was made to a modification of the spores sometimes observed in Peronospora. Mr. Earle of Cobden, Ill., collected species on Geranium and Viola, where, instead of the usual branching spore-stalks, the spores were borne on the mycelium close to the breathing-pores; the spores themselves being very much larger than in the common form. A similar monstrosity has been noted by Cornu in the grape-mould. The specimens were collected by Mr. Earle in April, and the speaker suggested that this form of spores might perhaps be an adaptation to the cold and wet weather of spring. The conditions which produce the monstrous forms are worth considering by collectors.

Of the diseases caused by Uredineæ which have advanced from west to east, the hollyhock-disease, *Puccinia malvacearum*, is the best-known instance. Its original home was probably Chili; but it spread through Europe about ten years ago, not, however, by way of this country, as was probably the case with the potato-rot. The diseases produced by fungi of other orders, as Ascomycetes, do not spread with the same rapidity as the rusts and rots. This is

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shown by the black knot, which is so destructive in this country to plums and some kinds of cherries. It is a native of this country, and is found on most of our wild species of Prunus, especially the choke-cherry, a shrub which has been introduced into many places in Europe. As yet, however, the black knot has not made its appearance in Europe.

The speaker then said he had just found the grape-mildew growing on the Virginia creeper (*Ampelopsis quinquefolia*) near Minneapolis. As this plant is closely related to the vine, the occurrence of the mildew might have been expected. In attempting to prevent the spread of the disease to countries where it is unknown, the discovery is of importance. It is evident, that, to prevent the spread of the disease, the importation of Ampelopsis as well as of grape-vines must be prohibited.

LEAVES OF THE GRAMINEÆ WITH CLOSED SHEATHS. By W. J. BEAL of Lansing, Mich.

[ABSTRACT.]

AUTHORS speak of the leaves of Cyperaceæ as having closed sheaths, while those of the Gramineæ have open sheaths. It is true, at even a very early stage, that many of the leaves of the Gramineæ have open sheaths, but all of our common grasses, so far as examined, have one or more of the lower sheaths closed. In many cases these lower sheaths are split or torn open as the stem and succeeding leaves enlarge. The same is true of the lower leaves of the branches. Some twenty species of grasses, mostly natives of North America, have been examined.

In case of *Arrhenatherum avenaceum*, three or more of the lower leaves have closed sheaths while there are are five or more in *Dactylis glomerata*. Species have been examined belonging to Alopecurus, Phleum, Poa, Festuca, Horedum, Colamagrostis, Lolium, Setaria, Panicum, Secale, Avena, Eragrostis, Sporobolus.

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Development of a dandelion Flower. By John M. Coulter of Crawfordsville, Indiana.

[ABSTRACT.]

The paper gives, as a result of personal investigation, testimony upon which are based the following conclusions in respect to this species.

1. The inferior ovary is produced by an arrest in the development of the floral axis, the rising in a peripheral ring of the floral organs, and the gradual arching over of the cavity thus produced by two carpellary leaves.

2. The syngenesious anthers are united by contact and pressure, but in no sense structurally.

3. The ovule is not produced directly from the axis, but is an outgrowth from the surface (probably the midrib) of a carpellary leaf.

A Fact bearing upon the Evolution of the Genus Cypripedium. By E. S. Bastin of Chicago, Ill.

[ABSTRACT.]

In June 1881, while botanizing at the southern end of Lake Michigan, I found a specimen of *Cypripedium spectabile* that was peculiarly interesting. I described and figured it at the time in "The Druggist," but it seems to me to be of so much significance in connection with the evolution of the Orchidaceæ in general and of Cypripedium in particular that I have thought best to bring it to the attention of this Association. The monstrosity to which I allude was an almost regular flower growing on the same stem with one of the ordinary form. In the usual form, the calyx consists of two whitish, broadly oval, obtuse, or scarcely pointed sepals, the lower one evidently composed of two united pieces. The corolla consists of two lateral white petals about as long as the sepals, but narrower. The lip occupies the position of the third petal and is a large and sac-like organ, often two inches long, pinkish, except on the posterior under portion, and decidedly pink on the anterior upper portion. The sides of the oblong aperture of the lip are

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provided on either side with an ear-like infolding. The gynandrous stamens are two in number and lateral, opposite the petals. The anthers face backward toward the column, which is declined. Occupying the upper part of the column, in nearly the position of the third stamen, is a large dilated-triangular, somewhat petal-like body which curves downward toward the interior of the lip and covers the stigma and stamens. The style is bent downward at right angles to the column and is about two-thirds as long. The stigma is dilated-triangular, obscurely and unequally three-lobed, facing toward the interior of the lip, and is not seen until the latter organ is bent downward, owing to the fact that it is covered by the broad rudimentary stamen. The ovary is inferior, six-ribbed, somewhat curved and twisted and one-celled with parietal placentæ.

The monstrous form which I found differed from this in possessing three broadly lanceolate sepals all alike and not at all united. It had no lip, but three regularly formed pure white petals all of the same size and shape, as long as, but somewhat narrower than, the sepals and alternating with them. Representing the first staminal whorl were two instead of one fleshy dilated triangular bodies occupying the normal position, *i. e.*, alternating with the petals. They resembled in thickness and in general shape the one in the normal flower and were also spotted like it, but were somewhat smaller and not bent but erect or nearly so. The second staminal whorl consisted of three fully developed stamens inserted on the column opposite the petals and closely resembling those of the normal flower in structure, except that coalescence with the style was less complete, and the connective, or projection at the back of the anther, was rather more conspicuous. The stigma was nearly equally and deeply three-lobed, the lobes alternating with the stamens. The column was but slightly bent, the ovary scarcely twisted and the flower was nearly erect. It will be observed that here, in a genus of plants affording some of the most strikingly irregular flowers in the vegetable kingdom, was a flower all but regular, and unsymmetrical only with respect of one thing, viz.: in not possessing even a vestige of the third stamen of the first staminal whorl.

If the specimen is to be regarded (as undoubtedly it should be) as an instance of atavism or reversion to an ancestral type, we have here a striking confirmation of the view botanists have reached

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of the typical structure of the flower of a monocotyledon, viz.: that it is a trimerous, regular and symmetrical flower possessing five whorls of floral organs. It tends also to establish the conclusion already reached by Robert Brown and other able botanists that the large dilated-triangular organ of the ordinary flower is the rudiment of a stamen belonging to the first staminal whorl, a view, which plausible as it seems on theoretical grounds, has nevertheless had but few facts for its support. No doubt the organ originated by the disappearance of the anther and the broadening and thickening of the filament and its extension, the connective. In fact, in the monstrosity under consideration the fertile stamens were when viewed from the back, very much like miniatures of the two rudiments of the outer whorl.

But the study of this monstrosity seems to throw some light also on the nature and origin of the lip of Cypripedium, and hence, by inference also, of that of other Orchidaceæ. Mr. Darwin, from a study of the arrangement of the spiral vessels in the lip of Cypripedium has reached the conclusion that this organ is a compound one, made up of the lower petal, two stamens of the outer, and one of the inner whorl. This view would seem a very plausible one indeed aside from the evidence furnished by the grouping of the spiral vessels, for it accounts at once for the disappearance of parts, for the enormous development of the lip, and for the two curious earlike infoldings of the upper margin. But if this monstrosity truly represents a stage in the development of Cypripedium, I do not see how this view can be maintained, for one stamen of the outer whorl had entirely disappeared and no trace was left behind, or any evidence from a difference in the size or thickness or venation of the petals that it had coalesced with one of them.

Then also if Mr. Darwin's view were the correct one, that the two stamens of the outer whorl had gone to form the outer part of the lip and the earlike infoldings, they would both have disappeared or coalesced with the third petal at the same time, but this specimen indicates that they disappeared one at a time. We must conclude, then, that the lip, if a compound organ at all, is made up merely of the lower petal united to the stamen of the inner whorl that would normally come opposite to it, and Dr. Crüger is right in denying that the homologies of the parts of flowers may safely be deduced from the course and grouping of the spiral vessels.

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THE POSITION OF THE COMPOSITÆ IN THE NATURAL SYSTEM. By JOS. F. JAMES of Cincinnati, Ohio.

[ABSTRACT.[1]]

AN examination of the structure of the flowers of the Compositæ, with a suggestion that the Gamopetalæ should be considered as of a higher type than the Polypetalæ; and that the Compositæ should be placed at the head of the vegetable kingdom.

PERIODICITY OF SABBATIA ANGULARIS. By MARY E. MURTFELDT of Kirkwood, Mo.

ON THE TRITUBERCULATE TYPE OF MOLAR TOOTH IN THE MAMMALIA. By E. D. COPE of Philadelphia, Pa.

[ABSTRACT.]

IT is now apparent that the type of superior molar tooth which predominated during the Puerco epoch was triangular or tritubercular; that is, with one internal and two external, tubercles.[2] Thus of sixty-seven species of placental Mammalia of which the superior molars are known, all but four have three tubercles of the crown, and of the remaining sixty-three all are triangular excepting those of three species of Periptychus, and one of Ectoconus, which have a small supplementary lobe on each side of the median principal inner tubercle.

This fact is important as indicating the mode of development of the various types of superior molar teeth, on which we have not heretofore had clear light. In the first place this type of molar exists to-day only in the insectivorous and carnivorous Marsupialia; in the Creodonta, and the tubercular molars of such Carnivora as possess them (excepting the plantigrades). In the Ungulates its persistence is to be found in the molars of the Coryphodontidæ

[1] The paper is printed in full in the American Naturalist for December, 1883.
[2] See American Naturalist, April, 1883, page 407.

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of the Wasatch, and Dinocerata of the Bridger Eocenes. In later epochs it is occasionally seen only in the last superior molar.

It is also evident that the quadritubercular molar is derived from the tritubercular by the addition of a lobe of the inner part of a cingulum of the posterior base of the crown. Transitional states are seen in some of the Periptychidæ (*Anisonchus*) and in the sectorials of the Procyonidæ.

The tritubercular or triangular superior molar is associated with a corresponding form of the anterior part of the inferior molar. This kind of inferior molar I have called the "tubercular sectorial,"[3] and is very variable as to the degree of development of the sectorial cutting edge. The anterior triangle is formed by the connection by ridge or crest, of the median and anterior internal cusps with the anterior external. Its primitive form is seen in Didelphys, Pelycodus, Pantolambda and the Amblypoda generally; in Centetes and Talpa; and in its sectorial form, in Stypolophus and Oxyæna, etc.

The mechanical action of such teeth is as follows. Of course it results from the form of the superior molars that the spaces between them are wedge shaped, the apex external, the base opening to the palate. The base of the triangular section of the anterior part of the inferior molar is interior, and the apex exterior; and when the jaws are closed, this triangular prism exactly fits the space between the superior molars. The lower heel of the inferior molar receives the impact of the crown of the superior molar. Thus the oblique edges of the inferior triangle shear on the edges of two adjacent superior molars. The anterior parts of the inferior molars form, with the superior molars, an alternating dental series, as distinguished from the prevalent opposed dentition of most mammalia. In so far it resembles the reptilian dentition.

This primitive dentition has been modified in two directions: viz., to form the grinding and sectorial dentitions. As already remarked, the superior molars gradually acquire a posterior internal lobe, which produces the quadrituberculate type. This lobe, by opposing the anterior internal lobe of the next posterior inferior molar, precludes the entrance of the anterior triangle of the latter between the two superior molars. Hence we find in the types which possess quadritubercular superior molars that the anterior triangle of the in-

[3] See Report G. M. Wheeler, to chief of Engineers on Expl. Surv., W. 100th Mer. Vol. iv, pt. ii, on the Creodonta.

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ferior molar is not elevated, if present, as for instance in Rhinoceros. It is, however, more frequently atrophied, and disappears, forming the inferior quadritubercular molar so well known.

On the other hand, as I have pointed out[4], the anterior internal cusp of the triangle of the inferior molar may be more developed antero-posteriorly, giving the antero-external edge of the triangle much greater obliquity than the postero-external. In correspondence with this modification, the superior triangular molar loses its equilateral character by the more anterior position of its internal angle, thus elongating the posterior internal side. The latter thus fits the corresponding form of the triangle of the inferior molar, forming with it the shear of the sectorial tooth.

In a former article "On the Homologies of the Molar Teeth, etc.,"[5] I traced the modifications of the superior and many of the inferior molars of the Ungulate mammals to a parent quadritubercular type. In a subsequent essay[6] I traced the origin of the inferior sectorial to a primitive five-tubercled or "tubercular sectorial" type. Farther than this I did not go, and made no attempt to derive the few cases of triangular superior molars then known, nor the type of the superior sectorial. The revelations of the Puerco fauna show, that the superior molars of both ungulate and unguiculate mammalia have been derived from a tritubercular type; and that the inferior true molars of both have been derived from a "tubercular sectorial" type. Shall we look for the origin of the latter in a tritubercular tooth also, *i.e.*, the tubercular sectorial without heel; and will the crowns of the true molars of the primitive mammals alternate with, instead of oppose, each other?

On the Structure of the Skull in Diclonius mirabilis, a Laramie Dinosaurian. By Edw. D. Cope of Philadelphia, Pa.

[ABSTRACT.]

This paper was based on a nearly perfect skeleton, found by Messrs. Wortman and Hill, in Dakota, of the *Diclonius mirabilis* of Leidy, a species closely related to the *Hadrosaurus foulkei*. The total length of the skeleton is thirty-eight feet; that of the

[4] On the origin of the sectorial tooth of the Carnivora. American Naturalist, 1875.
[5] Journal Academy Nat. Sciences, Philadelphia, March, 1874.
[6] On the origin of the sectorial tooth of the Carnivora. American Naturalist, 1875.

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skull 1.18 meters. The skull is much "like that of a goose." The dentition is remarkable for its complexity and for the difference between the superior and inferior series; it includes more teeth than the Hadrosaurus, there being 568 in each maxillary bone, and 378 in each splenial bone, making in all 1,892. Dermal or corneous structures have left distinct traces in the soft sandstone about the end of the beak-like muzzle. The affinity of the Dinosaurs to birds is confirmed by the skeleton, but it is empirical rather than essential, and is confined to a few points, as the form and position of the vomer, the large development of the premaxillary bone, and the toothless character of this bone and the dentary. So far as the author is aware, the extension of the premaxillary above the maxillary, so far as to overlap the lachrymal, is unique among Vertebrata.

ON TWO PRIMITIVE TYPES OF UNGULATÆ. By E. D. COPE of Philadelphia, Pa.

[ABSTRACT.]

EXHIBITIONS of figures of restorations of *Phenacodis vortmani* and *Hyracotherium venticolum*.

PHARYNGEAL RESPIRATION IN THE SOFT-SHELLED TURTLE (ASPIDONECTES SPINIFER). By SIMON H. GAGE of Ithaca, N. Y.

[ABSTRACT.]

DURING the last twenty-five years, the mechanism of respiration in the Chelonia has been investigated with considerable thoroughness both in this country and in Europe; and at present the Chelonian form of respiration is considered to be comparable with that of the mammal rather than with that of the frog, as formerly supposed. While, however, the mechanism of respiration has been quite fully investigated, there has been, so far as I am aware, but one who has considered the organs of respiration in the different groups of turtles.

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Professor Agassiz, in part II, of the "Contributions to North American Zoology" (p. 284), states that the lung capacity of the soft-shelled turtle is far less in proportion to its body weight than is that of the land turtles. He also states, in considering this fact, that the skin on the ventral side of the body, from its rich network of blood vessels, acts as a respiratory organ. He further states that in the pharynx are many fringelike processes which resemble the inner gills of tadpoles, and probably have the same function, although no mention is made as to the method of their use.

In 1878 while watching a soft-shelled turtle from Cayuga Lake confined in a glass aquarium, it was observed that the throat and the floor of the mouth became alternately swollen and collapsed, while the turtle was completely immersed in the water. The appearance was very much like the respiratory movements of a frog in the air. As no air escaped from the turtle the bulging of the throat and mouth must be caused by filling the mouth and pharynx with water, and expelling it, or the air must be forced into the mouth from the lungs and then forced back into the lungs, as is done sometimes by men when swimming under water.

In order to determine whether or not water was taken into the mouth and expelled, the bottom of the aquarium was covered with fine sea sand, and the observations were made when the animal was resting quietly on the sand.

At the beginning of the movement, the mouth would slightly open, and its floor would swell out, the swelling passing steadily onward to the throat. After a moment of quiet, the swelling would disappear in the inverse order of its appearance.

During the disappearance of the swelling of the throat and mouth, the sand for a considerable distance in front of the animal's head, would be swept aside as by a rapid stream. The movement of the sand, without the escape of air, seemed to prove conclusively that the mouth and throat were alternately filled with water and emptied.

These pharyngeal respirations, as they may be called, were very regular occurring ten to fifteen times a minute. My observations were verified by Professor Wilder and several of our laboratory students.

While therefore the investigations of Agassiz showed that in the pharynx of the soft-shelled turtle were organs apparently suitable for aquatic respiration, the observations here recorded of the

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rhythmical bathing of these organs with fresh water seem to make the evidence complete, that a true aquatic is combined with an aërial respiration.

It is hoped that during the coming year, investigations may be completed which shall determine the exact amount of oxygen consumed in this pharyngeal respiration, and the structure of this unusual respiratory organ in the soft-shelled turtle.

ON THE USE OF VASELINE TO PREVENT THE LOSS OF ALCOHOL FROM SPECIMEN JARS. By BURT G. WILDER and SIMON H. GAGE of Ithaca, N. Y.

THE petroleum preparation known as vaseline is known to be practically unaffected by ordinary temperatures and by most substances. In the Journal of the Chemical Society, July, 1882, p. 786, it is said to be "sparingly soluble in cold strong alcohol, and completely in hot, but separates out on cooling." After trying various substances,—wax, paraffine, oil and glycerine with but partial success, the use of vaseline was suggested by the two authors independently and nearly at the same time. The experiments tried this spring indicate that, during three months, at ordinary spring and summer temperatures, there is no appreciable loss of 95 per cent. alcohol from glass vials or jars, whether upright or inverted or on the side, provided corks are anointed on the bottom as well as on the side, provided ground glass stoppers are anointed and firmly inserted, and provided the rubber rings of fruit jars and the specimen jars made by Whitall, Tatum and Co., are anointed on both sides and the covers well screwed down.

We have also used the vaseline for preventing the loss of other liquids, excepting chloroform and spirits of turpentine; as a lubricator of drawers, and to prevent the sticking of the covers or stoppers of cement vials; and for the prevention of rust upon steel instruments.

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THE PSYLLIDÆ OF THE UNITED STATES. By C. V. RILEY of Washington, D. C.

[ABSTRACT.]

THE Psyllidæ, or flea-lice, are rather small homopterous insects, that have remarkable jumping powers. Some of them injure cultivated plants. This is notably true of the *Psylla pyri*, which blights the buds of pear-trees; and *Phylloplecta tripunctata*, which crumbles the tips of the blackberry. The family has received little attention in the United States, and scarcely anything has been known of the life-history and development of the species. The paper enumerates seventeen described species, four of these being synonymes, and one of them (*Psylla pyri*) introduced from Europe. They fall into four subfamilies, and represent four genera already characterized, and three new genera,—Brachylivia, Pachypsylla, and Phylloplecta. The new species characterized are *Calophya vitreipennis*, from Arizona; *C. nigripennis*, on *Rhus copallina; C. flavida*, on *Rhus glabra; Pachypsylla celtidis-curcurbita*, forming galls on leaves of *Celtis texana*, *P. c.-pubescens*, *P. c.-asteriscus; P. c.-umbilicus* and *P. c.-vesiculum* — all forming galls on leaves of *Celtis occidentalis; Blastophysa* (nov. gen.) *c.-gemma*, forming galls on the twigs of the same tree; *Ceropsylla* (nov. gen.) *sideroxyli*, a remarkable form developing in pits on the leaves of *Sideroxylon masticodendron; Trioza sanguinosa*, on *Pinus australis; T. sonchi*, on *Sonchus arvensis;* and *Rhinopsylla schwarzii*, from the cypress-swamps of Florida. The paper records discoveries as to the life-history of the species, and especially those affecting Rhus and Celtis; the latter forming a group peculiar to North America, and the most perfect gall-makers in the family.

The eggs of Psyllidæ are attached to leaves by a pedicel, are somewhat pointed at one end, and often terminate in a filament. The young are broad and flattened, with a fringed margin. They are generally pale, and more or less covered with a flocculent secretion. Those on sumach are dark, and without such flocculence. Those making galls on hackberry have stout spines at the end of the body, by the aid of which they are able to work out of their galls.

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SOME RECENT DISCOVERIES IN REFERENCE TO PHYLLOXERA. By C. V. RILEY of Washington, D. C.

[ABSTRACT.]

EVERY new fact in the life-history of the insects of this genus has an exceptional interest, because of its bearing on the destructive grapevine Phylloxera. The genus is most largely represented in this country by a number of gall-making species on our different hickories, and the full annual life-cycle of none of them has hitherto been traced. The galls are produced, for the most part, in early spring; the winged females issue therefrom in early summer; thenceforth, for the remainder of the year, the whereabouts of the insect has been a mystery. The author has for several years endeavored to solve this mystery and at last the stem-mother (the founder of the gall), the winged agamic females (issue of the stem-mother), the eggs (of two sizes) from these winged females, the sexed individuals from these eggs, and the single impregnated egg from the true female, have been traced in several species. There is some evidence, though not yet absolutely conclusive, that this impregnated egg hatches exceptionally the same season; also, of a summer root-inhabiting life. In *Phylloxera spinosa*, which forms a large roseate somewhat spinose gall on *Carya alba*, and which has been most closely studied, the impregnated egg is laid in all sorts of crevices upon the twigs and bark and in the old galls, in which last case they fall to the ground.

Up to this time they have remained unhatched, and will in all probability not hatch till next spring, thus corresponding to the "winter egg" of the grape Phylloxera.

ENTOMOLOGICAL NOTES FOR 1883. By E. W. CLAYPOLE of Akron, Ohio.

[ABSTRACT.]

THE POTATO BEETLE.—This insect, in 1882, came as usual in Middle Pennsylvania in the early summer. I was compelled to use poison as in previous years. In the later portion of the summer I

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observed and noted at the time, in the Canadian Entomologist, that there was no second brood or that it was so small as to pass unnoticed. It was my intention to watch in 1883 in order to determine if this second brood was again missing. But to my surprise in 1883 there was almost no first brood. The pest was gone. Here and there a few beetles appeared, or later a few brood of young were observed, but they were so few that by most persons they passed unnoticed. All the season the plants grew and flourished uninjured by their enemy.

Being called away on business to New York in May, I observed the same fact over the whole intervening country. Everywhere the potato beetle was conspicuous by its absence. In every part of Pennsylvania I find the same result, at least as far as I have enquired, and letters from my former residence in Ohio inform me that there too the plants are free from their enemy.

I know not to what cause this freedom is to be ascribed, but note the fact, for want of time has prevented my making any investigation. But whatever it is, I am disposed to date its action in the middle or early part of last summer, and not during the winter as popular opinion inclines to believe.

THE HESSIAN FLY.—During the present season, 1883, the Hessian fly has been exceedingly abundant in some parts of Perry Co., Pa. I saw many fields in which the crop would certainly not pay the cost of cutting it. I have found four, five, six and seven "flaxseeds" in a single straw. This of itself is not deserving of special remark. But on enquiry I was told by farmers that it was not as is usually the case the earliest wheat that was infested. Several assured me that in their own and their neighbors' crops, the earliest and latest wheat was free, and the rest of the crop was attacked. By "earliest" I mean wheat sown between Sept. 1 and Sept. 7, and by "latest" that which was sown after Sept. 22. I do not know if this fact has been generally observed, but I am satisfied the statement is true for Perry County.

In explanation, I can only suggest that probably the earliest wheat does not escape the fly, but being large and strong, is better able to bear it, and though suffering injury yet sprouts and repairs its loss before the spring growth comes on. The same reason — stronger growth — may also enable it to resist the spring attack.

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PSEPHENUS LECONTEI; THE EXTERNAL ANATOMY OF THE LARVA. By D. S. KELLICOTT of Buffalo, N. Y.

[ABSTRACT.]

THE species referred to is found in large numbers at the rapids above the falls of Niagara, and is scattered throughout the northeastern part of North America. The author proposed to supplement the accounts given of it by earlier observers with a record of his own observations, which differed in some respects from those of Dr. LeConte. Several details of anatomical structure were brought to the attention of the members, and illustrated with wood-cuts prepared for the purpose and with specimens mounted in balsam for observations under the microscope.

NOTE ON PHYTOPTIDÆ. By HERBERT OSBORN of Ames, Iowa.

[ABSTRACT.]

THE Phytoptidæ comprise a group of very minute mites, species of which produce galls of various forms on the leaves or twigs of various trees. Recent investigation in Europe has placed the group in a different light from that in which it previously was considered. Their study is rendered difficult by their extreme minuteness, and the care necessary to discover the different stages. One of the most common species produces the little wart-like swellings which occur so abundantly on soft maple leaves. A species on ash leaves produces a swelling which is nearly uniform on the upper and under surfaces of the leaf; while another species on the same tree produces a leafy growth at the end of the twigs, the growth sometimes being inhabited also by cecidomyian larvæ. On the elm occurs a large deformed leafy growth, which also contains Phytopti; while still another form of gall occurs on box elder, consisting of a depression on the under surface of the leaf, this depression being filled with a woolly growth, and containing Phytopti.

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Fossil Cephalopoda in the Museum of Comparative Zoology.[1]

By Alpheus Hyatt of Cambridge, Mass.

The nature of the evidence afforded by fossil shells is very little understood. They have been so often spoken of slightingly, as a sort of jacket, an unimportant part, etc., that all conclusions arrived at by their study alone are considered as peculiarly liable to error.

A shell to begin with ranks as a primary, essential part, and from its mode of growth it is a model of the external form, and at the same time a mould of the outlines of the internal soft parts to an extent which has not been fully appreciated. The shell may often become a permanent record of the series of changes, which the form has undergone, from the time it first began to inclose the embryo until the death of the soft parts. Among Nautiloids and Ammonoids, it contains another part, also calcareous, the tube, or so-called siphon, which exhibits remarkable and significant changes of structure and position in the early stages. This connects the septa or horizontal partitions, which with their sutures vary with the age of the animal constituting a third record of early changes and structural modifications.

All these parts are found to be in correlation with each other, and together make, when properly translated, an index to the life history of the individual, which is unequalled in some respects among other existing or extinct animals.

A single shell, either from a living or fossil form, may present, accurately, the general history of the development of the young, the stages of the adult and of old age. The results of heredity and the action of endemic or traumatic diseases can be detected, if one knows how to study and compare the remarkable and distinct series of metamorphoses often displayed by this external or protective skeleton with those of congeneric forms.

The vertebrate skeleton has long been considered a standard, and the evidence afforded by its fossil remains is very important and convincing. The series made in the case of the horses found by Marsh and those described by Gaudry are universally

[1] Partial abstract from a memoir now in preparation, of which two other, but distinct abstracts containing different portions of the memoir have appeared since this was read. One, "Genera of Fossil Cephalopods" was published in Proc. Bost. Soc. Nat. Hist., V, 22, Dec., 1883 to Feb. 1884, and one "Evolution of Cephalopoda," in Science, Camb., Feb., 1884, nos. 52–53, V, 3.

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quoted as the strongest proofs of evolution. This evidence is considered complete, because naturalists understand and have thoroughly studied the skeleton, and because it is internal and has been assumed to be more invariable than the shell. All of these arguments have their due weight, but we should like to hear of an example of greater invariability than exists between the shells of the Nautilus now existing and those Barrandeoceras (Nautilus) of the Cambrian, or of the Triassic and Cambrian Orthoceras.

The conclusions arrived at by the study of the skeleton are reliable, but they really are no more conclusive in theoretical meaning than any other case of a series of equally well understood hard parts in any other branch of the animal kingdom found as fossils, and traced out in the same careful and thorough manner.

How unreasonable it would seem to a student of fossil mammalia, if he were requested to do what it would be appropriate to require from a student of the fossil Cephalopoda, viz., to describe from the investigation of the single perfect fossil skeleton of an adult, not only the characteristics of the skeleton at the stage of growth in which the animal died, but the developmental stages of this same skeleton, and in case it were the remains of an old, outgrown animal, also, the retrograde metamorphoses through which it had passed during its last stages of decline.

It might require a lifetime to make out the stages of a single species of mammal satisfactorily from the isolated specimens which could be found, and the attempt would be hopeless for all of the youngest periods of growth, while the bones were still cartilaginous.

Crustacea, especially Trilobites, afford an opportunity of doing this, as Barrande has shown, but the labor expended in gathering the necessary number of specimens demands the most extensive and persistent efforts, and one can rarely be perfectly sure that the same species is under observation all the time, since it is impossible, as among the Ammonites and Nautili and living animals, to follow out the same individual through all its transformations.

This kind of evidence, however, is as readily obtainable among fossil Cephalopods with relation to the shell and other hard parts as among living animals, and it can be obtained in good collections everywhere, whether "in situ" or in Museums. Thus it is possible to study the relations of these fossil forms very minutely and with a certainty of possessing a clew to their true relations,

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which is rarely attainable even among existing animals, for among these we have only the embryos and young of contemporaneous forms and necessarily lose all relations of succession in time, unless the investigation embraces a prolonged series of experiments or is more or less historical, and even then the facts cannot have a very wide chronological range.

The accepted divisions of the Cephalopods have been founded by authors wholly upon the characteristics of the adult. The form of the shell, whether straight as in Orthoceras, curved as in Cyrtoceras, coiled up with open whorls as in Gyroceras, or with the whorls in contact as in Nautilus. These modifications together with the outlines of the aperture and other minor characteristics, have, heretofore, determined the group to which any given shell was referred.

The examination of the young of all the closely coiled Nautiloidea shows them to be as a rule uncoiled, and in the earliest stages simply arcuate as in the adults of the group of the Cyrtoceratites, and having a scar on the apex which represents the beginning of the stage in which the animal commences to construct the true or secondary shell. The young of all the Ammonoids have, on the contrary, with the marked exception of some Palæozoic species and some *varieties of species*, closely coiled whorls at the corresponding stage of growth, and upon the apex is a tiny bag or embryo shell, which has been very appropriately called the protoconch by Owen.

In my Embryology of Cephalopods, Bulletin of Museum of Comparative Zoology, Cambridge, No. 5, Vol. III, the position was taken that the scar of the Nautiloidea showed that a protoconch had existed in the embryo of Nautilus, but had disappeared during the growth of the shell, the scar being uncovered by its removal. This supposition was endorsed by Prof. Richard Owen, but rejected by Barrande, who insisted that the general absence of a protoconch was a fatal objection. There exists, however, on the apex of some Orthoceratites, an excrescence or bulb of a withered and somewhat irregular and variable shape. DeKoninck, in his late magnificent work upon "Calcaire Carbonifere de Belgique," curiously enough cites these very forms as the principal proofs against my conclusion because in them the scar is absent. According to my examinations and drawings, however, the scar ought to be found in such examples underneath the bulb of the

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apparently complete apex, which is simply the withered and shrunken remains of the primitive protoconch. This was evidently originally a soft, embryonic shell composed of conchiolin and not of calcareous matter as in the Ammonoidea. I have seen and figured several examples in which a bulb was present on the apex and no scar visible, and one case in which the bulb (protoconch) had evidently been broken away leaving the scar visible below surrounded by the broken edges of the outermost shell layer which formerly connected the apex with and covered the protoconch. The external layer of shell and its longitudinal ridges from the apex up on to the so-called plug of the cicatrix described by Barrande, have also been traced and thus every point in the evidence appears to be complete, and the fact that the bulb is covered by a true protoconch continuous with the shell of the apex seems to be established.

Space is wanting to discuss the question of the convergence of the orders of Cephalopoda, but some of the facts will be given farther on. M. Barrande's strenuous and magnificent opposition to the view that these orders did converge in the Palæozoic rested largely upon the supposed differences of the embryo of Nautilus and Ammonites, and he figured several of the species, which carried the bulb or protoconch and described them as having the apex plugged or filled as in truncated shells. The figures referred to are in Barrande's Syst. Sil. de la Bohême, pl. 488, case 9, 11, *O. elegans* and *O. politum*, and the fact that the external shell is continuous may[2] be seen in specimens of *O. crotalum* in Mus. Geol. Surv., Albany.

The classification adopted is as follows:

CLASS CEPHALOPODA.

SUB-CLASS I, TETRABRANCHIATA.
Order 1, *Nautiloidea.*
" 2, *Ammonoidea.*

SUB-CLASS II, DIBRANCHIATA.
Order 3, *Belemnoidea.*
" 4, *Sepioidea.*

These four orders converge to one type by intermediate forms, by the embryology and development of the shells and internal hard

[2] Since this paper was read figures illustrating some of these facts have been published in Science, Cambridge, Mass., V, 3, No. 52, Feb. 1884.

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parts, by their morphology, and by their habits which may be inferred from the forms of the apertures and build of the shell. Judging from these facts, the prototype of the Mollusca must have had a globose embryo attached to the apex, the apex composed of a living chamber opening into the protoconch or globose shell of the embryo without septa, though possibly divided more or less by diaphragms. Diaphragms precede the formation of the septa in the embryo Ammonoid and a cone formed by them appended to the bottom of the cœcum was figured in the author's Embryology of Cephalopoda, Bull. M. C. Z., and described by M. Chalmas as the prosiphon.[3] This confirms Von Jhering's opinion that Tentaculites was the prototype of the Cephalopoda, since it has similar embryo and diaphragms. The prototype of the sub-classes I and II must have been a form of the same type, with however a single septum or series of septa having closed cœca in place of a siphon. These cœca were the initial stages of the funnels, parts of the thick, hard septa, and continuous with them and not the true siphon or in any way *inclosures at first* of the fleshy walls of the true siphon; they were directly in contact with the outer side of the apex of the mantle and strictly homologous with the internal partitions found in many Gasteropoda. The cœca, becoming prolonged in descendant forms, were differentiated into the funnels, the remnants of the cœca and the thinner walls of the sheath proper, connecting them were formed by the fleshy siphon. This is the condition of the siphon in the typical forms and in tubular siphoned Orthoceras, but in some aberrant genera the fleshy siphon widens near the living chamber becoming conical and forming a sheath. These sheaths lie in the large tube formed by the true funnels and may deposit permanent diaphragms as in Endoceras. The fleshy sheaths formed also a median tube connecting the apices of the sheaths and surrounded by a layer of shell which is shown in Piloceras in a structure first demonstrated by Dr. Dawson.

[3] These diaphragms, as figured by the author in the work cited pl. 1, f. 3, 4, pl. 2, f. 1, y, pl. 3, f. 3, 5–6, are apparently quite distinct from Meunier Chalmas' figures of the prosiphon which as drawn in Spirula and Ammonites in Fischer's "Manuel de Conchyliologie," 1881, pp. 336, 371, seems to be a distinct connective prolongation from the bottom of the cœcum across the vertical section of the protoconch. This prosiphon of Chalmas was not observed either by Branco or the author, though the former sought it in Spirula and many forms of Ammonitinæ. These facts suggest that the diaphragms which preceded the formation of the cœcum were variable in form and they are apparently the homologues of the calcareous plate which closes the cicatrix in Nautilus. If this be true they lose the value attributed to them by Chalmas and Fischer and do not indicate any close affinity between Spirula and the Ammonoidea.

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This, the endosiphon, had the same thin covering as the sheaths themselves or secondary diaphragms, and is but rarely well preserved. The sheaths were only temporarily formed and resorbed in some genera, Sannionites, better known as Cameroceras, and in Piloceras as suggested by Dawson.

The true Endoceras, *E. proteiforme* Hall, has numerous successive sheaths and the endosiphon in its most aberrant condition, and also the siphon composed only of the funnels. Sannionites and Piloceras have, as a rule, but one perfect sheath, this being appended to the living chamber; any others are apt to be imperfect and the remnants attached to the endosiphon, as in Dawson's figure of Piloceras, Can. Nat., V, X, No. 1, p. 3, f. 2.

It is interesting to note that all of these types have the aspect of having been indigenous. They are so numerous in all the lower formations, Calciferous and so-called Quebec, and in the corresponding rocks in northern Europe that they almost exclude the types having small siphons, thus affording the strongest contrast with the Bohemian faunas, and presenting a more ancient aspect than any of these, a fact already stated by Barrande. The endosiphonate and transition types of these periods have a common character, already noted by Billings in various specific descriptions and evidently understood by that author, though not as strongly insisted upon as its importance deserved. All of them, including also the coiled types, have the septa very closely set or what would be designated as crowded together, and the siphon central or between the centre and the ventrum, with the exception of a very few species. Individuals increase the distance between the septa by growth up to the adult stage, and then in old age the distance again decreases. This peculiarity is, therefore, important and characterizes the radical types existing in the more ancient formations as well as the young of the Nautiloids in general. It is also to be noted that the young of the Nautilus has the siphon larger than in subsequent stages. While this more ancient aspect of the faunas in North America is true of the formations antedating the Chazy and Trenton, and perhaps including the Lower Silurian or Cambrian as a whole, it is not true of subsequent formations or other types in these later periods. The Devonian Cephalopoda, as a rule, have the aspect of and belong largely to our Spyroceran group and are similar to the Bohemian Silurian Nautiloidea of the same series. The Goniatites of America have also the aspect of

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migrants and the radical Silurian forms abundant in Europe are wanting. The group of Clymninæ are also absent.[4]

M. Barrande has shown that all the different types of tubular-siphoned straight cones may be followed by means of transitional forms into the large-siphoned, or Endoceratitic and Actinoceratitic genera, and in other directions this tubular siphoned group may be followed into the nummuloidal-siphoned groups of straight forms. All of these have been divided into genera, which are described in the twenty-second volume of Proc. Bost. Soc. Nat. Hist., but these do not interest us here. It is necessary merely to restate M. Barrande's facts, and also to note the central position of the tubular-siphoned or typical straight cones. They connect, as proved by Barrande, in other directions, notably with the brevicone forms. From these last any one may trace the series, which leads through the brevicone Cyrtoceratites, described by Barrande, into the Gomphoceratidæ, in which the lower forms, abundant in this country, have open apertures and similar siphons. The next members of this series are the so-called Phragmoceratites and others with much contracted apertures. Gomphoceras also connects with the truncated Mesosceras of Barrande, which leads into the Ascoceratites.

A good illustration of this method of classification is shown by the frilled shells, which are a singularly homogeneous group, and about which the evidence is quite complete. The longicone straight shells have a genus Dawsonoceras composed of annulated cones which have a distinct frilled layer on the exterior. These frills at intervals stand out from the sides showing the marks of having been at one time the edges of apertures.

The *Cyrt. lamellosum* Hall, Amer. Mus., is the type of the arcuate group of this series in the Silurian, and then follows a large series of gyroceran Devonian forms leading into nautilian shells, also in the Devonian, Mus. Geol. Surv., Albany, of which the type is the *Gyr. undulatum* Hall.

A parallel series in the same family group begins in the Devonian, having the same style of shell, with the *Cyrt. Jason* Hall, and may be followed into gyroceran forms. In this series we have enjoyed the criticism and assistance of Prof. James Hall, who has stated somewhat similar facts in his Natural History of New York, with the exception of the connection between *Cyrt.*

[4] Have recently seen a specimen from Burlington, Vt., that may prove to belong to this group.

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lamellosum and the Orthoceratites, which we worked out when away from Albany.

Another fine series begins with *Orth. Archiaci* Barr., and can be traced by the forms, sutures, and position of siphon through *Lit. magnificum* Bill., of the Silurian, and then into a true nautilian shell, the *Naut. ponderosum* White, and into *Naut. bilobatum* Sow., and *Naut. ferratum* Owen, both in the Carboniferous. The dorsal lobe is introduced in *Naut. ponderosum* and in the succeeding forms it becomes divided by the reappearance of a small, median dorsal saddle.

In these, and many other instances, the evidence has been accumulated which enables us to show that the genera, Orthoceras, Cyrtoceras, Gyroceras and Nautilus, as those terms are now applied, are in reality generalized descriptive terms for representative forms of different series, which resemble each other in form, but are distinct structurally. Each series can be distinguished by its own differential, structural characteristics and by the form of the whorl in section, but each one springs from some straight or arcuate form, and passes through a parallel series of transformations which can be designated by the terms cyrtoceran, gyroceran and nautilian. By the examination of these series we have learned to distinguish accurately the nautilian forms, as those having an impressed dorsal zone due to close coiling, and in this impressed zone a dorsal lobe is developed sooner or later in the sutures. This distinction enabled us to point out the extreme rarity of true nautilian forms in the Silurian. Most of those, thus named, though closely coiled, have a gibbous dorsum and dorsal saddles in the sutures and are true Gyroceratites; for example, the major number of the species described by Barrande as Nautilus, and many American species, come into the gyroceran category.

Some arcuate forms have a very slight dorsal inflection, and flattened or slightly impressed dorsum. These do not contradict but confirm our evidence, and have been fully considered. They are either degraded descendants of true nautilian shells which have become uncoiled and show their descent in their closely coiled young, or else the dorsal lobe can be traced directly to the peculiar conformation and the excessive arcuation of the whorl.

If we turn from the series of adults to the study of the stages of growth in individuals, we find everywhere a similar repetition of the arcuate, gyroceran and nautilian forms. The first stage in

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any nautilian shell is a Cyrtoceras, the next is Gyroceras, and the third Nautilus; and almost equally invariably the development of the sutures begins with a dorsal saddle, and is followed by a dorsal (internal) lobe after the impressed zone is formed.

After the expiration of the Palæozoic, shells with large open umbilici disappear as stated by Barrande, and this statement, as we have previously argued in our Embryology of Cephalopods, denotes the decreased importance of the arcuate and gyroceran stages in the young. This opening in the umbilicus we have called the umbilical perforation, and shown that it was an important distinction. It is found in all nautiloids and not in the ammonoids except certain ancient larval forms. We know that the straight and arcuate cones and gyroceran forms have also in large part become extinct, or are dying out at the time of the Trias, so that both the parent forms and the indications of immediate descent from them, as shown in the development of individuals, disappear simultaneously.

The evidence, therefore, derived from the study of development in the individual, teaches that the Mesozoic forms are derived from shells which have been in themselves close coiled or nautilian forms. This is carried out fully by the facts of gradation as they subsequently occur in time. Thus the typical Nautilidæ of the Trias, like *Cenoceras* (*Naut.*) *carolinus* Mojsis., are directly traceable into the striated and smooth shelled truncated forms of the same period and of the Jura, like *Naut. truncatum* Sow., and we trace the latter by intermediate shells into the Radiati group of the Cretaceous. The genus Nautilus did not begin to exist until the Jura and all the supposed similarities, which have led to the suppositions of M. Barrande and others, that it is a genus closely allied with silurian nautilian shells, are errors arising from incomplete knowledge of the structure and development. Nowhere in this series is there a sign of immediate derivation from a cyrtoceran or gyroceran ancestor. The approximations of the adults are close and undeniable, and in the development the arcuate stage is reduced to smaller proportions and occupies less time in the development than in the preceding Palæozoic series. Exception must be made in favor of those forms in the Palæozoic, which crown series, and which by descent have gradually acquired small umbilical perforations; but these are by no means numerous and are serviceable to our hypothesis, since they stand out prominently as exceptions

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to the common run of palæozoic forms. A curious confirmation of this conclusion was found by the author in studying the development in time of the inner or dorsal lobe of the sutures. This lobe is, as we have said at first, in most Silurian Cyrtoceratites and Gyroceratites, and in the young of the nautilian forms preceded by a saddle or forward inflection of the dorsal sutures.

In Barrande's Nautili of the Silurian there are some with dorsal lobe, and in our American species it also occurs in company with the impressed zone, but in none of these, though no doubt it may occur in very rare examples, have we found the slightest indication of the small median, dorsal lobe, which we have called the annular lobe. In the true Nautilidæ of the Devonian, like *Nephriticeras* (*Naut.*) *bucinum* Hall, etc., the dorsal lobe acquires this addition in the shape of a median depression deepening the original shallow outline into a V-shape. In the Triassic and Jurassic forms the edge of the V-shaped lobe rises anteriorly and has a distinct, internal conical depression, the cone of the dorsal part of the septum. In the Radiati the adult never has the cone, but may have the lobe in the sutures. Nautilus, in the adolescent stage, has the cone and the sutural V-shaped lobe which I have called the annular lobe, but the cone is lost by growth, and the annular lobe alone is retained in the adult. In the Jura some adults may retain the cone, but most lose it as in Nautilus. In the Triassic forms all seem to have the cone until a late stage, and some certainly in the adults. The annular lobe also changes its form in the Jurassic species from V-shaped as in the Palæozoic to U-shaped as in modern Nautilus.

In other series of shells with distinct origins which may be traced in some series even more accurately and satisfactorily than in the above, a similar succession of phenomena has been observed. Nautilian shells in the palæozoic, until the close of the carboniferous, are generally traceable to cyrtoceran forms having similar characteristics of form, position of siphon, sutures, and aspect of surface; and by inference from these to Orthoceratites of different types.

The picture which this presents is somewhat startling at first, but we feel sure that after an examination of the facts naturalists will at any rate be disposed to respect our conclusion, that types among Cephalopoda, and in other branches of the animal kingdom, became more quickly changed in the earlier stages of their

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evolution than subsequently, and that this quicker evolution gave the aspect of sudden creations to these earlier forms as they appeared in the palæozoic and a distinct character to the palæozoic age as compared with subsequent ages. For example, the highest type of Nautiloidea is Subclymenia, a species with ventral V-shaped lobe and ventral siphon, so like Goniatites, that it can be separated only by its young and the gradations of forms which connect it with certain well-known Nautiloids. This springs up suddenly a species by itself in the Carboniferous, in a group which, altogether, are only of family importance and are directly traceable to arcuate forms. Many cases like this could be cited, notably M. Barrande's *Celœceras* (*Gon.*) *prœmaturum*, which is a highly complicated goniatite appearing before its proper time in an evolutionary sense, and cited by M. Barrande as a significant fact against any hypothesis of progression in time. An allied species has the adolescent stages like the adults of the Nautilini until nearly full grown, and then the sutures become suddenly complicated. This is the well known *Gon. hybridus* Munst.[5]

If any of these types occurred without their congeners in the earliest Cambrian we should have a similar picture to that presented by the Ammonoidea of these formations. These in a few cases present young which have the straight orthoceratitic character; these few cases are purely varietal and in the same species, as shown by M. Barrande, we may have the young with a straight apex or closely coiled. Sandberger and the author have added to this, instances of varieties of other species which are loosely coiled or gyroceran in form in the Devonian and in the Carboniferous, but so rare that Branco was unable to find a case, though he examined many embryos in this period. After the Carboniferous, as shown by Branco and the author, all Ammonoids are close coiled (unless it is found that the extreme degraded form of Baculites in some cases becomes straightened out again, a not improbable anticipation).

The evidence of the young of *Agoniatites* (*Gon.*) *fecundus* Barr., shows therefore, that the Ammonoidea had a pre-palæozoic history of quick development from straight Orthoceratitic forms similar to that of the Subclymenia and some other forms of Nautiloids. This like the *Centroceras* (*Discites*) *Marcellense* of Hall had ventral

[5] We are informed by Prof. Zittel that Munster's figure of this species is erroneous and that this comparison cannot be made.

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lobes and ventral siphons which have led many naturalists into the mistake of considering them as prototypes of Goniatites or else true Goniatites. In the Cambrian, the Ammonoidea had already attained a stage of evolution which was not reached by the Nautiloidea until after the expiration of the Palæozoic.

There are sufficient remnants of the earlier transition forms left in the Silurian to demonstrate the former connection of the Ammonoids and the Nautiloids. There is one group of slender whorled forms like *Bactrites* (*Orth.*) *pleurotomum* Barr., Syst. Sil., pl. 296, and others on pl. 297, which by their striæ of growth, their long funnels and the positions of the siphon which varies from the centre to near the ventrum, show close affinities with typical Bactrites. This last has sutures which in some cases are deeply arcuate and have both dorsal and ventral saddles, with laterally compressed elliptical whorls. This same form is repeated in the coiled whorls of *Mimoceras* (*Gon.*) *ambigena* Barr., and *compressus* Beyr., in both of which also the whorls, though often touching, are truly gyroceran. They have no impressed dorsal zone, and the sutures are precisely similar to the arcuate sutures of Bactrites. Mimoceras is not in any sense a true Ammonoid, except in the embryo, which has the protoconch as in that order. It is evident, therefore, that these Goniatites were not derived from Bactrites in the Silurian. The same form of protoconch may be found in Bactrites, but, so far as known, Bactrites is a true Nautiloid in its earliest stage and sheds the protoconch. The facts then are only sufficient to identify them as the remnants of transitional series which were once united in some slender form of tubular siphoned straight cones existing before the Cambrian.

The Goniatites mentioned lead directly into the true Nautilian forms of the Nautilinidæ whose transitions into the true Goniatites of the Devonian are by insensible gradations. Interesting as these are, we have no space for their consideration; we have given them in the Proc. Bost. Soc. Nat. Hist., v, 22, p. 303. This evidence is, however, sufficient to justify us in attributing the origin of the Goniatites to a series which began with a straight form and passed through in its evolution the arcuate, gyroceran and finally arrived at the nautilian stage. The Ammonoidea in the Cambrian are like the Nautiloidea in the Trias after the expiration of the Palæozoic and like them possess, with the exception of the few forms mentioned, exclusively nautilian shells. These shells

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also as we have stated being very close coiled with an impressed dorsum and dorsal lobe and possessing a slight depression on either side of the protoconch which is a remnant of the umbilical perforation. In this respect they are even more highly differentiated than the Nautiloidea of the Trias. The annular lobe also appears, first only in *Agoniatites tabuloides* sp. Barr. of the Silurian and becomes general only in Devonian forms. Thus the type of the Ammonoidea shows decisive gradations in differentiation of important structural characteristics and forms a single series parallel to one of the series of the Nautiloidea and therefore must have evolved with great comparative rapidity.

Instead of many series springing off on all sides from a common type as do the families of the Nautiloidea from Orthoceras, there seems to have been but one evolving directly from Orthoceras which develops similar forms and structures, but these becoming at once the radicals of still higher differentiations without passing through the preparatory stages which endure throughout the Palæozoic among Nautiloids. Pathological and uncoiled forms occur in limited localities in later formations up to the end of the Jura and then the whole type retrogrades. As shown by Neumayer the normal forms have simpler sutures and the uncoiled types also become more numerous and general and the whole type ceases with the termination of the Cretaceous.

During the Palæozoic, the Ammonoidea present an intermediate character in their mode of evolution. The types produced all have nautilian shells, but the genera are distinguished from each other by essential differences of the internal organs and sutures, and some groups like the Clymeninæ present wide departures from the normal outlines of the sutures, and the siphon itself in the last is displaced, being near the dorsum instead of ventral, as in all other Ammonoids. That Clymenia is an Ammonoid is now unquestionable, Branco having shown that the young were ammonoidal, and the author having confirmed this easily verifiable fact.

In the Trias such essential and wide structural differences begin to disappear and in the Jurassic we are presented with an almost homogeneous group which naturalists, until a very late date, have insisted upon including in one genus. This error is now, however, being widely corrected, but the differences of generic importance are not such decisive structural distinctions as are observed between genera of Palæozoic Goniatites. The sutures are much more

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homogeneous in Jurassic forms, and we have to rely to a great extent upon the ornamentation of the whorls and the less important differences shown by the digitations of the sutures, etc.

Both the Belemnoidea and Sepioidea confirm these conclusions, the facts pointing in each of these to an independent derivation from distinct types of Orthoceratites, the former from the true Orthoceras, and the latter from the flattened forms of which Gonioceras is the nearest in affinity now known.

The truncated shells of Orthoceras have been described by Barrande; and the plug which fills up and rounds off the broken apex, accounted for as an external product due probably to the action of two arms which he supposed stretched out posteriorly from the aperture as in Argonauta and embracing the whole shell and extending even beyond the broken end filled it up by successive deposits of carbonate of lime. We have also studied this structure and can confirm M. Barrande's observation, and the conclusion that the plug is an external product with a peculiar bilateral structure, but that it was deposited by two arms extending backwards we do not regard as probable. The markings indicate that at the truncated apex the layers were laid on by an organ which was certainly double at the end, but not necessarily double all the way up to the aperture. The body of the whorl in Orthoceras does not have any prolongations of the ventral and dorsal furrows of the plug, nor are there any longitudinal rows of spines or furrows, as in Argonauta, indicating the presence of two arms, capable of secreting shells, nor does the aperture with its entire outline give strong support to this proposition.

We think it is possible to strike nearer home in homology; the fold of the mantle in Nautilus satisfies the conditions. This is an active shell-secreting organ, which was certainly present and also functionally active in the Ammonoidea and Nautiloids, and probably more important in these ancient forms than it is now in the modern Nautilus. This is also more consistent with the explanation of the structure of the Belemnoid, which, as is easily seen in the famous examples of the preserved animal, had no such pair of enlarged arms, and yet deposited externally a solid covering, the guard, which is in our opinion the homologue of the solid filling of the truncated end of Orthoceras. Branco in his admirable paper on the structure of the curious Triassic Belemnoid, Aulacoceras, shows that the secreting organ of the guard must have been open

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and proves it by his drawings of masses of foreign matter included in the layers of the guard. This and the channel which runs along the ventral side in the guard and is often single, and generally more persistent and longer than the dorsal channel favor this view of the homologies. Quenstedt and Von Jhering have both traced the Belemnoids to Orthoceras, and this opinion supports their views and accounts for the preservation of the protoconch, both as a useful organ containing the siphonal cœcum in its neck and because of the protective guard built up around it at an early stage of growth. The tracing of Belemnoidea back to Orthoceras accounts for the dorsal and ventral channels, both of which are present in the plug of the truncated Orthoceras. It also permits us to explain the central hollow trace or tube in the guard of the Belemnoid, as the homologue of a similar hollow trace or tube, the pseudo siphuncle which invariably occupies the centre of the plug in truncated Orthoceratites, and has not been heretofore accounted for. The Belemnoids are nearer to the Nautiloids in the Trias than any of the later periods, since Aulacoceras is in some measure an intermediate type with a less perfect guard than that of the typical Belemnoids so abundant in later formations. The Sepioids are usually joined with the Belemnoids, as a closely approximated type, and this conclusion is carried out in a very general way by geological evidences. There is a well defined palæozoic group of Orthoceratites, in which the transverse section of the whorl is fusiform, Eudoceras of Hall; these grade into Gonioceras, a flattened shell with wide acute, lateral expansions like those of the shell in fossil Sepioids. One example in Professor Hall's collection is so remarkably similar to the dorsum of the common Sepia shell in shape and lines of growth, that it would have certainly misled the author, if the dorsal aspect alone had been visible. The sutures luckily prevented this mistake, but even they have a peculiar curvature not found in any type of Nautiloid, and yet agreeing remotely with the equally peculiar curvature of the layers of the internal filling, of calcareous, spongy matter, which is a notable characteristic of the Sepia shell. If these are transformed homologues of the septa, and we think they are, there is reason for associating the Sepioidea and Goniocerati-dæ, just as we above associated the truncated Orthoceratites and the Belemnoidea, though the series is by no means as perfect.

The Sepioidea are, however, a more highly concentrated type

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rapidly parting with the adult ancestral characteristics, and also the protoconch; reducing the shell on the ventrum to a narrow band and altering the septa to a series of continuous deposits, and finally in the existing Loliginæ, fusing the primitive shell-gland and shell-sac, and in the Octopods the shell finally altogether disappears.

Whatever is the fate of this opinion, the Sepioids do not directly approximate to the Belemnoids in past times, but are distinct and originated from the Orthoceratites. The absence of the external shell or protoconch in the young of the existing Sepioids and Octopods and the generation and final destruction first of the septa and then of the shell can be accounted for as due to their great differences of habitat and to the action of the law of concentration in their development.

This position is also supported by Lankester's observations on the embryology of the Sepioidea, in which the pen sack of the mantle in Loligo is accounted for as a secondary formation and not the primitive shell-gland, but is nevertheless supposed by him to have become confluent with the true shell-gland. The remarkable embryological insight which prompted his view and led him to refer to the sac as probably due to the transformations occurring in the Belemnite, is exactly fulfilled by Aulacoceras, and sustains this hypothesis. Thus when we meet with an internal shell in the animals of the group Sepioidea, we must imagine the shell as still enclosed in the hood, although the edges of the once opened sac formed by the hood have become permanently united. Fusion with the primitive shell-gland would follow naturally the inclusion of the shell of Aulacoceras and Belemnites in a flap or hood-like prolongation of the mantle forming a pen-sac. The union of this sac with the primitive shell-gland is a natural sequence, which follows from the law of concentration in development as soon as the inheritance of the shell-sac and its tendency to close became fixed in the adult structure and inheritable by the embryo of Sepioidea.

If this be true, all the types of the Cephalopoda probably originated in the Palæozoic, and our conclusions accord with those of Barrande among various types of invertebrates, and other authors, notably Alexander Agassiz among Echinoderms. In fact the vertebrata alone present several distinct types, possibly of Mesozoic origin, and these are by no means certainly of that date in all cases, and there are only comparatively few types of the inverte-

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brata which cannot be easily proved to have originated in or before that age. The tendency of modern discovery has been to carry back the origin of types to more and more ancient dates. Thus we find Cope claiming among his remarkable Dyassic (Permian) reptiles that the Theromorpha are not only the radicals of the Reptilia but also combine characteristics of the Batrachia and Mammalia, especially the Monotremata, and finally that the vertebræ are amphicœlian and distinctly notochordal.

The palæozoic, therefore, presents itself to us as essentially the birthplace of types, and at the same time as the age during which great structural differences originated with a rapidity equalled but rarely in later times.

The pre-palæozoic is unknown, but the inference holds for this age also, and there is great probability, that in this period the evolution of forms was proportionally quicker. This view is not affected by the theory of the alternation of continents in order to account for the accumulation of sediments, and provide homes for the evolution of types before they make their appearance in the known formations of this or other continents. It is simply as we have tried to show an inference drawn from the positive data of the series studied, and consequently applies to the phenomena of evolution in every location.

In the Cambrian, as is now well known through the labors of several palæontologists, the advent of types is sudden, and they are in themselves numerous, but in our experience the same law holds for succeeding periods. If we select the formations in which forms appear, we usually find them spreading out with wonderful rapidity, sometimes without any apparent precursors, and sometimes with radical forms immediately preceding their appearance. Our impression derived from the minuter study of series among Cephalopoda is not that the evolution of groups was slow, but on the contrary excessively rapid, and that the groups, if presented graphically, would form fan-shaped figures instead of long branches.

A notable and well known example is the descent of the larger number of the Liassic Ammonites, the Arietidæ from *Psiloceras planorbis*, a fact established by the observations of Quenstedt and confirmed by the subsequent experience of several authors and the special investigations of the writer. The family radical is here one species closely approximate in time to the family itself, which

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is composed of several distinct genera and different series of more complicated forms. These must have spread out with great rapidity in the successive faunas of the Lower Lias. The same is true of the larger number of all groups.

The inference, therefore, is, that types near their point of origin are more plastic and more easily changed by the forces acting upon their generalized structures.

If we adopt the hypothesis that habits forced upon the animal by the habitat are the principal means of modification, and lay aside the teleological notion that every change must in some sense be an advantageous difference requiring long periods of time for the action of natural selection, the law of battle and the survival of the strongest, etc., we can understand the origin of the orders of Cephalopoda and the curious phenomenon of the quick evolution of their successive forms.

Thus the Tetrabranchiata are crawlers and leapers, and but rarely swimmers. Their shells show this in the forms of the apertures and outlines of their whorls. The ventral sinus in the aperture, when large, indicates that the animal possessed a well-developed ambulatory organ, the so-called fleshy siphon or funnel of recent Nautilus and of the Sepioidea. When this is moderately developed and the aperture open, we can be quite sure that the animal was furnished with powerful arms for supporting the shell, and was a crawler mainly. Such were the straight shells without exception, and most of the Cyrtoceran forms, the young of the Nautiloids with coiled shells, and the open apertured adults of the same: that is, by far the larger number of all the Nautiloidea.

When the aperture was contracted so as to present unusual development of the ventral sinus through which the ambulatory tube was extruded and a narrow aperture across the centre, but at the same time a broad opening for the extension of dorsal arms, the inference is quite reasonable that we have to deal with the remains of a powerful swimmer, and that the animal was perhaps capable of crawling, but certainly not so well fitted to hold the shell in an upright position as the open apertured types. The Gomphoceratites and Phramoceratites have this description of aperture, the T-shaped.

When, as in Mesoceras and our American Billingsites, the aperture is contracted abdomino-dorsally, and elongated transversely,

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forming a dumb-bell-like outline, and all traces of the ventral sinus have disappeared and the shells are habitually truncated, we should infer that the animal must have been some sort of a climber or clinger, perhaps to the stems of plants, or other animals, but could not have been a ready crawler or a good swimmer.

Among the Ammonoidea we find also a variety of apertural modifications, which are interesting, but for the most part these were open apertured. Many of the Goniatites of the Palæozoic possessed the ventral sinus large, and striæ of growth similar in curvature to the Nautiloids, showing them to have had similar habits, but in the Ammonitinæ of the Mesozoic we find a significant change. The rostrum appears, and the siphon as an ambulatory organ must have almost wholly disappeared, leading us, almost, to the unavoidable conclusion that they were more confined to a crawling littoral life than any of their ancestral Palæozoic forms.

Finally, in the shepherd's crook of the living chamber of the Cretaceous Ancyloceras, one of the degraded forms above alluded to, we find an aperture turned in upon itself, having no signs of any ambulatory powers whatever; in fact, we can imagine no way in which this creature could have progressed with comfortable celerity. We can only imagine it as stationary, either as buried with the coiled end in the mud and cleaning the surrounding space for a living, or else hanging among plants with the mouth upwards and catching what was brought by the current within reach.

A curious verification of these views exists in Argonauta, one of the naked Octopods. The egg case of the female is not a true shell, *i. e.*, it does not arise at the start in a shell-gland, but is a secondary product of the arms and mantle. Nevertheless, it occupies the position and has the same relations to the body as the true shell of Nautilus, and is a hard unyielding case composed of layers of calcareous matter, and the opinions we have advanced above would certainly be seriously shaken if it did not resemble the shell of Nautilus. It belongs to an animal having a large ambulatory tube and a good swimmer, and it has consequently exactly the structure that we should expect under these circumstances. The form is similar to Nautilus and Goniatites, having a large ventral sinus and striæ of growth with similar curvature, but the microscopical structure perfectly distinct and indicative of its independent origin.

The guard of the Belemnoidea, which we regard as the further

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development of the plug of the Orthoceras, is a weighty solid bar and in a cylindrical body, such as these animals are known to have had, could not have aided them in floating. That it could have, as supposed by some palæontologists, been of use like the pen of Loligo, as a sort of spring in the back, to help them sustain the frequent shocks against solid objects in their backward leaps, is highly improbable. Nature never takes the equivalent of a stiff iron bar for such uses, unless it is external, and there is a protective cushion of flesh next the animal. The only inference here seems to be, that the animal of the Belemnoidea was a ground swimmer and leaper, and the guard was used to keep it on the bottom, or else as an aid to bore into the mud, or for some similar purposes.

The ground habitat is intermediate in character between the surface swimming habitat of the Sepioidea, and the crawling habitat of the Orthoceras. According to our hypothesis the structure of Belemnoids also should be intermediate between these two types. It follows also that this intermediate structure might occur independently in the Belemnoidea, and that they were not, as their structure seems to imply, transition types between the Sepioids and Orthoceras. All palæontologists have noticed the retention of the shell, which, though internal, still resembles closely that of Orthoceras, and this alone is sufficient, even if it is not admitted that the guard is the homologue of the plug in Orthoceras, to prove our statement, and separate them from the Sepioidea. Spirulirostra of the Tertiary in which the guard is much reduced was probably a free swimmer and Spirula in which the guard was wholly lost appears from the disk on the end of the mantle to have been, as first stated by Rumphius, an animal capable of attaching itself either to floating objects or other animals.

The Sepioidea are, for the most part, expert swimmers, and the loss of the shell by the later forms occurs in accordance with this hypothesis. It first becomes an open shell, with only the dorsal side preserved in the adults, at the same time the siphon being lost, and the septa changing into a light porous mass in the interior; and lastly, in the Octopods, it is wholly lost.

Characteristics are divisible, according to our observations, into two categories, which have, heretofore, except by a few writers, not been separated. They are however widely separated in their history, in the same or different types.

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We have already alluded to the independent production of the arcuate, gyroceran and nautilian forms and of the dorsal, annular and ventral lobes, and ventral siphon in several types of Nautiloids, and in the earlier Ammonoids. We have now to call attention to the fact that in each case these peculiarities could not have been inherited from the radical type Orthoceras, since they do not exist actually in this form any more than the adult bird or mammal exists in the cell which becomes an egg, or the egg which becomes an adult by growth. The transformations presented by each distinct series, however, are precisely parallel and possess the same aspect of inevitable sequence which is observable in the development of eggs into adults of the same species. Thus in the nautilian shells of the Mesozoic, whether Nautiloids or Ammonoids, if we study the smaller families and divisions within each order, we can show definitely that the lower or radical ancestral shells are almost invariably discoidal and that they become progressively more and more involute in the progressive series.

These facts and those previously noted with regard to the development of the nautilian coiled shell independently in each series, notwithstanding the separate origin of these genetic series in corresponding but distinct orthoceran forms, are of the same class of phenomena and in the same line, greater involution being simply closer coiling. We have then perfect confidence in declaring that, given the form of shell, whether straight, arcuate or coiled, with which a series may begin, if a progressive series it will necessarily produce in succession more arcuate, then loose coiled or gyroceran, then close coiled or nautilian, and finally more and more closely coiled or involute shells wherever it occurs without reference to locality, or the formation in time. That the structure progresses in the same ratio with the coiling has been demonstrated by several writers beginning with Von Buch.

There are however, as is well known, species of Nautiloids and Ammonoids which become uncoiled. These forms are close coiled in their young and show conclusively by this peculiarity and the history of their adult gradations that they are descended from close coiled or nautilian forms. They are degraded forms and this can be proved not only directly by their gradations and development, but indirectly by comparing their forms, sutures, etc., with the transformations taking place in the old age of normal nautilian shells. There is the same inevitable sequence in their evolu-

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tion of form, but we must begin with a higher form instead of a lower one and in most series we have to start with a nautilian shell and say that in a retrogressive series it will inevitably evolve in course of time through the more and more uncoiled forms and finally end in a straight form.

The parallel series of forms occur only in types having similar habitats and habits; they are independent of locality and time, but not of the more general physical requirements of the habitat. They are equally strong between the Silurian and Mesozoic and modern Nautiloids, and it is from not understanding this close representation in distinct genetic series, that has led Barrande and others into erroneous statements with regard to the close affinities of the modern and the Cambrian Nautiloidea, which are purely representative forms, essentially distinct in structure and derivation, and their resemblances independent of heredity. No parallelisms occur between types in different orders having distinct habitats. There are no parallelisms or independent reproductions of similar forms in different genetic series between the Belemnoidea and Nautiloidea or Ammonoidea, as there are between the last two when compared with each other. The two swimming types, however, Belemnoidea and Sepioidea, present parallelisms of a similar kind and a succession of forms which can be closely compared.

Representative forms and characteristics appear to us therefore as direct products of similar general requirements of the habitat acting upon a radical, or common organization.

The differential characteristics are quite distinct in behavior. The old idea that Ammonoids may be distinguished from Nautiloids by the ventral siphon and ventral lobe requires modification. As a rule it is true; but as we have stated, and all paleontologists know, there are exceptions. The constant difference consists in the fact that the siphon never breaks the outline of the suture of the ventral lobe in Nautiloidea, whereas in the Ammonoidea the nearness of the siphon to the ventrum causes a slight saddle in the suture, the siphonal saddle. In descendant forms this saddle remains as a constant characteristic and is found in the young at an early stage and even independent of the nearness of the siphon to venter. In the Ammonoids, also, the so-called inverted funnel of the siphon appears. This, according to our observations, is an additional part or siphonal collar and exists in some forms of Goniatites with the funnel below. It arises at first in our opinion from

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the passage of the siphon through the ventral suture and then becomes fixed in the organization in the same way as the siphonal saddle in the sutures. They are in our eyes both accounted for as purely mechanical results, but being once established the conservative tendencies of heredity perpetuate them independently of each other. Nothoceras among Nautiloids has the funnel actually reversed according to Barrande, but this must not be confused with the collar above described, which has a distinct relation to the siphon, as is easily demonstrated by the microscope.

These differences are begun and maintained in all the progressive forms, and are not repeated in the Nautiloidea; so also is the division of the lobes of the sutures by marginal lobes and saddles, and other differences in smaller series are manifested in the same way.

We are perfectly willing to admit, that in this class of characteristics the Darwinian hypothesis receives the fullest confirmation. They are variable and transient often when first introduced and seem in every way to indicate the action of natural selection: that is, in our way of stating this law, as essentially the action and reaction of the animals one upon another. In the determinate succession of the representative characteristics, however, we can see only the action of the *purely physical selection* of the surroundings, and think they show that a wide distinction is to be made between the purely mechanical mode of producing modifications, physical selection, and the action and reaction of organisms upon each other, or natural selection.

All of the above remarks relate entirely to the subsequent history of characteristics after they have originated, and not to their origin. We have been able to find no indications of a difference in origin between characteristics. To us they all appear in obedience to purely physical selection or action of the surroundings upon the habits of the animals, and to this we have been able to find no objections in the facts themselves which have not been dissipated by subsequent study.

This impression has become so strong and the facts we think so convincing as to permit formulation. They have led us to regard all characteristics as probably indirect or direct responses to external stimulation, as structural equivalents of the requirements of the surroundings.

It is impossible here to make any statements adequately sup-

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porting this view, but we can fortunately refer to two writers who have arrived at very similar conclusions. Dr. Dorhn has discovered one of the most important laws of change yet formulated; namely, that organs possess latent capacities for the performance of other functions than those for which they are immediately used. Thus any change of habit may bring about the development of these latent capacities, which reacting upon the organ may entirely change its structure. Our researches lead us to adopt this view and it seems to us to throw light upon the whole question of the relation of the Orthoceratites and their descendant orders.

John A. Ryder has originated the theory of the mechanical genesis of tooth forms from the direct action of strains in using the teeth upon certain kinds of food, perhaps the best illustration of the modern view of mechanical theory of evolution, which has as yet been published. This writer also uses the term mechanical evolution in the same sense that we do, and evidently regards animal reactions or effort as an important factor in the origin of characteristics.

We find it impossible to separate now the work of Prof. Cope and Mr. Ryder clearly from each other, or our own work from that of either of these gentlemen. All we can say is, that if we have appropriated any of their ideas without proper credit, it is because we have worked them out independently, and if they serve to confirm their opinions we shall be most happy to make due restitution.

Prof. Cope also has insisted upon the necessity of bringing in the action of the organism as an important factor and regards the efforts of the animal itself as the initial stage of the origin of characteristics. This law of effort was also formulated by the author independently, but considered as a purely mechanical reaction of the organism in response to the physical requirements of the habitat and used to account for the presence of representative forms in distinct genetic series. The common organization of the radical would necessarily respond to the common requirements of the same habitat by similar changes of form and similar structural modifications. Thus while we find and must necessarily expect to find representation between the Nautiloidea and Ammonoidea in the same habitat and between Belemnoidea and Sepioidea, also in the same habitat, we do not find representation between the Tetrabranchiata as a whole and the Dibranchiata, because though originating from the same common radicals they diverge into different habitats.

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This representation we have designated as morphological equivalence because the forms and structures are equivalents of each other and the organic equivalents of the physical surroundings.

Instead of being led by our researches to adopt the Darwinian hypothesis that animals have an inherent tendency to vary, we deny emphatically that animals have any inherent tendencies except those of growth and heredity. The essential character of heredity is conservation, and if not perpetually acted upon by physical selection, we should never have had any variations or any animal kingdom.

There is evident to us throughout the history of the Cephalopods a persistent retention of type characters, and in the development a perpetual renewal of ancient characteristics which are unquestionably due to heredity, and are continuous from the earliest times. Any supposition, that the same force which accomplished this could also and at the same time act in opposition to itself or have any tendency like a tendency to variation, is illogical and inadmissible. That physical selection could originate characteristics, and that these should be eventually adopted and transmitted by heredity, is a sufficient explanation of the phenomena. It is not at all requisite to imagine heredity with a double capacity or that animals had any inherent tendencies whatever except the tendencies to grow and to reproduce their own characteristics in their young.

The contemplated publication strives also to show as in previous essays, that all characteristics arise primarily in the adults of any given type, and then tend to be inherited at earlier and earlier stages in the young of succeeding generations of all grades whether individuals or varieties, or species. In this respect the author is now able to quote besides those of Prof. Cope, the confirmatory observations of Würtenburger who has arrived at precisely similar conclusions with regard to the Ammonitinæ of the Upper Jura and also Haeckel and Balfour. Though differing from these authors and from Prof. Cope with regard to the part played by natural selection, we all agree, that such phenomena as the quick development of characteristics are common and can be represented as a general law of development.

Prof. W. K. Brooks has lately published an able work upon "Heredity" in which he adopts the law of natural selection, and the mixed definition of heredity. The author attributes different roles to the different sexes. Thus he regards the female as con-

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servative, and possessing less inclination to vary and therefore the main agent in maintaining the characters of the type, and the male as more variable, and, therefore, the immediate agent in any of the changes, or transformations of the original type characters which take place. This theory has many notable facts in its favor, but it will be difficult to apply it in forms where there are but slight, or no distinctions between the sexes, and again to the lower branches of animals in some of which hermaphroditism is prevalent as it is among sponges and Protozoa.[6] Prof. Brooks, we think, has in his theory mistaken a general result for a cause. The habits of the male being more varied, he is naturally more variable while the habits of the female being more uniform she is less variable, seems to us a reasonable explanation of the differences of the sexes. The laws of heredity would secure the transmission of the variations in whatever sex they occurred.

Again, throughout long series, like Nautiloids and Ammonoids, there are no signs of such action and we have as yet not succeeded among the fossils in distinguishing the sexes. According to Prof. Brooks' theory we ought to be able to do this and also to see that the males varied from the females in those characteristics in which the later and higher forms of both sexes vary from the older and ancestral forms.

Prof. Brooks has apparently found nothing worthy of his attention in the works of Cope, Ryder, Packard or the author, though he quotes from Mivart and others upon the subject of the quick evolution of forms. He also regards the fossils as very unreliable means of information in this respect, a point of view, which is founded upon time-honored prejudice, but where will the zoologist and embryologist get relations in time or any sufficiently extended information about the succession of forms, without a resort to fossils? Among Cephalopods, for example, the information is in these respects more perfect than any living group, as such, can possibly afford to any student, even though he should attain complete acquaintance with its structure and forms and their development.

We have said above that the changes occurring in the old age of individuals are true metamorphoses, and insisted that these had a meaning of their own, and correlated with the changes which occur in retrogressive series. Thus in an old shell the whorl

[6] See "Larval Theory of the Origin of Tissue." Proc. Bost. Soc. Nat. History, Vol. 23, not yet published, also Science, 1884, No. 58, and American Nat., May, 1884.

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tends to become uncoiled, loses its costæ, etc., and this is paralleled by the uncoiled genera and species of the groups of the diseased and distorted types of the Jura and Cretaceous.

These correlations are just as evident as the correspondences of embryology and the metamorphoses of the young with the adults of ancestral forms. It only requires study and attention on the part of naturalists for the laws of geratology to become as instructive with regard to the origin and development of retrogressive and distorted types (aberrant as they are now misnamed), as are those of embryology for the progressive and healthy types.

In other words there is a branch of pathology, which is applicable to the explanation of phenomena occurring as a normal condition in every animal in old age; and this, when carried farther, shows us that there are such things as pathological, diseased species, distorted types, retrogressive series. These are all apparently of similar nature and eventually die out by reason of their pathological condition, but yet exist long enough to reproduce their own awkward distortions, in genera, and series of genera. The author upon these grounds in 1866 formulated the law that there was an exact correspondence between the life of an individual Cephalopod and the group to which it belonged: namely, the young and adolescent stages having direct correspondence and repeating the past history of its own group to a greater or less extent, the adult corresponding to the present with all the peculiarities and differences of its group, and the metamorphoses of old age to the pathological modifications and changes found in the types which arose in unfavorable localities, or which were found as a rule to terminate the history of the group in time. It was also stated that the correspondences of geratology were quite distinct from those of embryology. While the former are due to heredity, the latter in all cases are due to the action of unfavorable surroundings. They are in the same category as representative characteristics and plainly the effects of similar causes acting upon similar organisms.

We have been until lately ignorant of the fact, that Haeckel had given expression to a very similar idea and had applied it to the whole animal kingdom in the same year 1866.

We are glad to be able to quote this eminent author, and to accord to him the greater knowledge and wider application of the law, which we felt at the time of our first publication was a com-

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pletion of the labors of Von Baer and Louis Agassiz upon the correlations of individual development, and that of the history of the group in time. We take all the greater pleasure in this act from the fact that Haeckel has received most strenuous criticisms, and his great services to science demand a more impartial acknowledgment than they have yet received.

The action of physical changes in the surroundings takes effect upon the irritable organism and this necessarily responds to the external stimulants by an internal effort. This effort, working from within and carrying all the influences of heredity, acts in its turn upon the parts and organs producing new growths and changes and these modifications are necessarily adapted or suited to the requirements of the habitat or the physical causes which incited the effort. Thus springing from the common radical we may have distinct genetic groups which diverge by reason of transmitted differences and yet are parallel, or have similar forms and correlated structures produced in similar succession in each group.

These morphological equivalents are general in their distribution with regard to space and time, and the causes must have been equally general and equally similar.[7]

All morphological equivalents seem to us to be sufficiently accounted for without the aid of natural selection, since the obvious fact that they are advantageous to the animals is a necessary result of their adaptability, a consequence of their *physical selection*. They are advantageous in the same way that it is of advantage to a drop of liquid to be round, or a cell when equally pressed on all sides to be hexagonal.

Differential characteristics, however, are only at first identical. After their origin we find them occurring in limited series of forms, appearing and disappearing according to no traceable law of succession, limited also in space and time, and having a fixed hereditary continuity and comparative invariability, which exhibit how exclusively they have become the property of heredity.

There is a time in each type, however, when they appeared as variables, when they were found in some, and not in other closely

[7] It must be observed here that we have carefully avoided mentioning a large number of characteristics such as the bilateral symmetry of the shell and the unsymmetry of many spirals, etc., which are, as we have tried to show in a previous essay, due to the still more general and uniform action of gravity. These are also morphological equivalents, but we have not the space even to allude to them as their importance deserves.

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allied forms. We can understand that, at this time, the organisms possessing them are able in their struggle with others to maintain and perpetuate themselves with greater success on account of these advantageous differences, and that by this success the differences would naturally become fixed in the organism and be adopted by heredity. Then they would assume the character of essential differences and the comparative invariability which distinguishes this class of characteristics from morphological equivalents.

Natural selection does not act according to our experience beyond the period of variability. All characteristics, according to the law of concentration, tend to be inherited at earlier and earlier stages in descendants, and the differences, when fixed are no longer variables and come under the full influence of the conservative force of heredity. They are not dependent upon the fate of battle, but are part of the invariable tendencies of the organism, so long as it remains in the same habitat. They may disappear or be dropped in some of the last of a series, out of the development of the young, in accordance with the law of concentration of development, or they may be retained, but in all cases they show the behavior of strictly hereditary characteristics of the class of differentials and serve to distinguish groups because of their comparative invariability of occurrence. They are the purest illustrations of the law, that like tends to produce like. One has only to think of the characteristics mentioned above as differentials in the Ammonoidea to understand these statements. The embryosac or protoconch, the ventral suture with its peculiar lobe, the nautilian form are all perpetuated steadily without break from the Cambrian to the Cretaceous inclusive, and the Nautiloid differentials persist in that order even in the existing animals. Similarly, in any small series, if we can pick out its differentials, we are presented with the same phenomena of comparative invariability due to heredity. In fact our classification of organic beings entirely depends upon this fact. If it were not true, we could not make a classification, which would represent natural relations. The methods pursued by all classifiers show this. While we talk freely of the need of founding classifications upon all the characteristics of animals, no one practically follows this empty wisdom. On the contrary, and of necessity, naturalists have been wiser than their own precepts, and really sought for those constant differences, which are the hereditary differentials.

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We do not regard the law of concentration as an inherent tendency of heredity. We think it is universal, and that it can be traced in man, as well as the Cephalopods, to an extent which throws light on his intellectual as well as his structural development, but it appears to be a result of the action of external stimulation upon the development.

Balfour not expecting, as an embryologist, to get any information from a paleontologist naturally omitted to notice, that in a memoir upon Planorbis at Steinheim, we had already attributed the concentrated or quicker development of some types to extra protection afforded by the parents to the young, and repeated our conclusions in his "Comparative Embryology."

His observations are valuable confirmations, and we can point to this as probably one at least of the causes of concentrated or quick development. It is however not the sole cause. Perhaps no types among Cephalopoda are more concentrated or have quicker development in Balfour's use of that term than the pathological type like Ancyloceras, Crioceras, etc., and that they had extra protection from their parents is an inadmissible supposition. They are of all grades, and diseased varieties may be followed into normal series or species on the one hand, and on the other by gradations into series of distorted forms with highly concentrated young. If these were protected the normal forms into which they grade must have been capable of affording extra protection to some varieties, while not possessing it themselves. Another fact is that their shells are the smallest in diameter, mere tubes in many genera.

All our observations agree however in one important particular: protection is certainly in its effect upon the young a stimulant, and may be said to furnish the most favorable conditions for an early and premature physiological development. It will not astonish pathologists, when it is asserted, that in this respect, protection is like the action of certain pathological causes which tend to produce premature development.

In fact we think, that while the causes which can have produced a tendency to concentration are almost entirely dissimilar and irreconcilable in themselves, they nevertheless possess the general and common character of being very favorable or stimulating to the earlier stages of development and unfavorable to growth. They belong in other words to the class of pathological

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causes and when in excess are highly unfavorable to the continuance of life itself.

Their normal action in these retrogressive and distorted forms is unfavorable; whereas, when stimulation arises from the ordinary action of physical selection, it may be entirely favorable and we think this is the only reasonable explanation of the phenomena.

Würtenburger and Haeckel both regard characteristics as tending to be inherited at earlier or later stages in descendants. If Würtenburger and Haeckel are correct then we ought to find series with tendency to inherit ancestral characteristics at later stages, or isolated forms with characteristics appearing at later stages than those in which they appeared in their ancestors. We have entirely failed to find any such instances. If they can be adduced, then the law of concentration would, as claimed by Haeckel and Würtenburger, be a result of natural selection. Neither Haeckel, Würtenburger or Darwin, has brought forward any such examples, so far as we know, and the latter is particular in stating, that characteristics tend to be inherited in successive descendants *at the same stage or earlier than that in which they appeared in the ancestors.* We can readily understand how characteristics may tend to be inherited at the same stage because of the invariable action of heredity, but we recognize no inherent tendency to change in any phenomena of heredity we have yet studied. The tendency to earlier inheritance is the result apparently of disturbing and modifying agencies acting from without as must all other causes of change in the organism.

The Argonauta is an example of an animal protecting its young and having a concentrated, or, as Balfour and others call it, quicker development. Many of the Spatangoids are similar examples. Man is the most remarkable example, since here we can with Cope trace the effect of protection not only in the nursing of the young, but in the subsequent care of rearing, and in education as its consequent.

In pathological types the development is enormously accelerated, as in the senile characteristics of syphilitic children, and the pathological types of Ammonites. The Baculites and uncoiled cretaceous Ammonitinæ, for example, are prematurely stimulated in the development of their senile characteristics by pathological causes.

In fact the only way to account for the removal of the progres-

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sive characteristics of the normal ancestors, and their replacement by senile characteristics, is to attribute the phenomena to the action of the stimulating effects of the surroundings, just as we are obliged to attribute the premature development of syphilitic children, or premature developments and extra growths of all kinds, to overstimulation by the unfavorable surroundings acting directly on the parents and indirectly through heredity on the children.

Thus Balfour's idea of protection, as applied to the so-called quicker development of the highest forms in different types, cannot possibly apply to diseased forms, or, as we might show if there was space, to parasites, which are however the finest possible illustrations of the effects of the surroundings, in producing concentration without protection. Nor does it apply to the uniform and constant manifestation of the law of concentration which takes place in every series. The highest members, *i. e.*, the terminal ones of series, are invariably more or less concentrated, and when the development skips stages, which are common in ancestral forms, as it does almost invariably in such forms or species, we cannot appeal to protection as an explanation. The skipping is evidently the direct consequent of the action of the law of concentration. The characteristics skipped have become useless; and as both Würtenburger and the author among Ammonoids, Cope among the reptiles and batrachians, and Haeckel more generally, translate the phenomena, they appear to be literally crowded out of the development by the earlier inheritance of the immediate ancestral characteristics. For example, the highest form of the Arietidæ is a small species, *Asteroceras Collenotii*, which has no stage in its development comparable with the adult of its ancestral *Asteroceras obtusum*, and yet we traced it, after several years of research, back through other forms directly to this species, and in this we have repeated the previous experience of Quenstedt and Prof. Fraas. Every series of Ammonites gives similar results, which have been noted in essays by Würtenberger and the author and the writers above mentioned in larger series.

The types with concentrated development thus skipping stages in smaller series are precisely comparable with the diseased types in their relations to the old age of the individual in the larger series: they present the same phemomena as the larger series of retrogressive forms in a less degree, but still perfectly traceable by definite

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characteristics. They lose the ornaments which distinguish the adults of the purely progressive species in their own series, and have smooth whorls; they usually also have less robust whorls, and the shell is not so large in diameter; they often retain in respect to their smoothness and the complications of the sutures, the conditions of their own younger stages throughout life, and thus present close parallels to the purely retrogressive forms of pathological series; they are also invariably either the dying-out forms of their respective series, or when on the same level the extreme aberrant forms.

Dr. C. S. Minot's essay,[8] Is man the highest animal? shows this mingling of characteristics clearly in the highest type of the animal kingdom, the mingling being due to the retention embryonic characteristics. This retention in a higher type, is a form of degradation which tends to destroy the adult differentiations of the type. Thus the excessive and premature development of the brain in man has prevented the development of the differential, simian characteristics of prognathism, reducing the bones of the face and increasing the size and importance of the skull. We can in the case of man, clearly see that the highest physiological development tends to destroy the growth or development of purely structural differentiations of the ancestral type. Geratologous types, therefore, present the phenomena of retrogression, but have also, frequently the highest possible developments of some of the characteristics and tendencies. Thus in *Asteroceras Collenotii*, the whorl is flatter and more acute and more involute than in any other form of its series or of the family of the Arietidæ, and the keel is developed earlier. In some series, on the other hand, as in the Stephanoceran, such forms may grade into those which belong to distorted, retrogressive series like Scaphites. We think therefore we have every reason to consider them and the retrogressive forms as having in common direct correlations with the retrograde metamorphoses of the individual, and consequently feel justified in classifying them in the same category as geratologous forms. Nevertheless, here as elsewhere no hard and fast lines can be drawn, and, strange and even paradoxical as it may seem, they must be in many instances regarded like man, as the highest of their series on account of their concentrated and accelerated development, and sometimes, as in *Asteroceras Collenotii*, exhibit the most

[8] Proc. Am. Assoc. Vol. xxx, Cincinnati Meeting.

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extraordinary development of certain tendencies, observable but not so highly developed in their ancestors.

They are also in smaller series often comparable with their earlier ancestors, just as the diseased Baculite as the terminal form of Ammonoidea is comparable with the Orthoceras. Thus the *A. Collenotii* can be compared in its later adolescent stages very closely with the adult of the ancestral radical of the family of the Arietidæ, *Psiloceras planorbis*, and still more closely with the immediate radical of its own genetic series *Agassiceras striaries*, which in some varieties is often confused with *Psiloceras planorbis*, and in the adult still retains the smooth whorl of that species. The extreme old age of this species Collenotii has never been seen, but if it reduces the whorl in size at that stage as others do in the Arietidæ, we should not be able to separate it from Psilocerας if the adult was unknown. The mingling of the progressive and retrogressive tendencies in the development of such forms as occur at the termini or paracme of small series, as Haeckel terms the descent and fall of forms, makes these comparisons less apparent than they are often between the purely retrogressive forms in the paracme of larger series, but that they belong to the same class is evident. The differences lie in the possession by geratologous forms of small series of a greater or less number of progressive characteristics which are developed, and prevent the tendency to the development of the senile characteristics. In the above case, for example, they permit the senile characteristic of smoothness, and also the simple acute, channelless abdomen of the young to be perpetuated in growth, but they develop otherwise a highly involute, laterally flattened whorl, which is a step in advance of all others of its own series.

All of the geratologous degraded characteristics are also similar to the larval characteristics, and consequently when the progressive characteristics disappear, we get as a result fusion of geratologous with larval characteristics, or persistent development of the latter combined with geratologous tendencies.

In purely retrogressive forms or purely geratologous types like Baculites, etc., the fusion of characteristics is complete, and the overstimulation of the pathological surroundings has to be called into account for the premature development of all the larval characteristics, the cylindrical form of the adolescent young, its smoothness, its simple sutures and the earlier inheritance of the

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tendency to degenerative uncoiling. We are, therefore, really presented, in Baculites, with a type comparable closely with the straight cone of the larval stage of *Gon. fecundus* of the Silurian, and with the primal adult ancestor Orthoceras, the resemblance to the radical being more complete because the process leading to the reversion is more completely carried out. The distal ancestor Orthoceras passes, as we know, through but few changes in its individual development, and has, what could be appropriately called, the direct mode of development. The higher types of the Nautiloids have the cyrtoceran, and gyroceran stages and the correlative metamorphoses of the septa and siphon in their younger stages, and have the indirect mode of development, or pass through a larger number of metamorphoses; the geratologous types have again the direct mode, on account of the skipping of progressive characteristics, or through their replacement by the senile tendencies and persistent larval characteristics. The purely adaptive nature of the indirect mode of development, which has been most fully studied among insects, is quite generally admitted by entomologists, and is prolonged or shortened according to the seasonal requirements of the surroundings, and may be regarded, in the main if not wholly, as the structural equivalent of the surroundings.

We have tried to show, that the more or less direct development of the geratologous forms among the Ammonites was possibly due to the stimulating effects of the surroundings producing premature and constant development of some of the senile characteristics in combination with the earlier development of the high progressive characteristics of the series or division, or where stimulation was excessive, replacing the latter entirely with the larval characteristics, and geratologous tendencies to uncoiling, thus producing larval types with direct development out of the ancestral progressive types. We have also tried to give sufficient reasons for our opinion that in all of these types the marks of effort are as apparent in the retrogressive as in the progressive series. This result is as clearly due to the habits of the aberrant or uncoiled types, as is the ordinary phenomenon of progression, but there is not space to discuss this point, and must refer to article in Science, 1884, No. 53, p. 148.

We can formulate our conclusions as follows:—

1. We have endeavored to demonstrate, that a natural classifi-

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cation may be made by means of a system of analysis in which the individual is the unit of comparison, because its life in all its phases, morphological and physiological, healthy or pathological, embryo, larva, adolescent, adult and old, correlates with the morphological and physiological history of the group to which it belongs.

2. In the different genetic series of a type derived from one ancestral stock, there is a perpetual recurrence of similar forms in similar succession, which are usually called representative and often falsely classified together, though they really belong to divergent, genetic series.

3. These forms and their similar characteristics are not derived by direct inheritance from the common ancestor in which all the forms are necessarily similar and primitive, but originate everywhere independently of hereditary influences in the different series, and also in all formations independently of chronological distribution.

4. This evolution of similar morphological changes in the forms of different genetic series must be regarded as the similar reactions or efforts of a common organism in direct response to similar, generally distributed physical causes active in the same habitat, and are, therefore, necessarily similar to each other though in different genetic series. As a whole, they may be said to express the general tendencies of modification, due to the efforts of the common-radical and common organization while spreading in all directions and in different genetic lines to respond to similar physical causes, and meet their requirements with suitable changes. They are, therefore, structural equivalents of each other in different series, and functional equivalents of the general requirements of the environment, or habitat, purely physical selections.

5. All new characteristics, even those which are purely mechanical reactions of the tissues, arise in a similar manner as reactions to the exciting agency of the more general, or more localized physical causes, and are necessarily their corresponding, organic or suitable equivalents, structurally and functionally.

6. After their origin, however, and during their subsequent history, characteristics are divisible into two categories: those which become morphological equivalents, and are essentially similar in distinct series, and those which are essentially different in distinct series and may be classed as morphological differentials.

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7. These differentials are absent in the first members of series, then on their appearance transient, but afterwards tend to become invariable, or fixed in the stock or series, being perpetuated and increased by direct inheritance in successive generations, species, etc. They may finally often disappear in the retrogressive or highest and last occurring members of each series, or in aberrant forms when on the same level.

8. They have no determinate mode of succession, but are usually more or less isolated modifications and arise first in individuals or varieties, but afterwards become characteristic of species and finally of the major part of the direct line in every descendant series.

9. They are, therefore, strictly adaptive, variable characteristics and not directed in their occurrence or development by any more or less invariable law of succession as are the morphological equivalents, and their perpetuation cannot be attributed to physical selection. Their rise and ultimate prepotency as inheritable characteristics appear to be due to the action and reaction of the animals upon each other, viz., to the law of natural selection.

10. Differentials, therefore, can be separated from other characteristics of the same parts, by careful observation and close analysis of their behavior in series, but cannot be specifically predicted from the study of other series; whereas, morphological equivalents can be predicted with the same certainty as the recurrence of any other physical phenomena.

Thus we can say of any new series of Nautiloids or Ammonoids, that habitat, *i. e.*, causes remaining similar, and the form with which the series begins being also known, they will, whenever or wherever found, tend to develop arcuate, coiled, close coiled or discoidal and finally involute forms in progressive series, and reverse this process in retrogressive series.

11. All modifications and variations in progressive series tend to appear first in the adolescent or adult stages of growth, and then to be inherited in successive descendants at earlier and earlier stages according to the law of concentration, until they either become embryonic, or are crowded out of the organization, and replaced in the development by characteristics of later origin.

12. Modifications which tend to appear in the old age of the individual of the progressive series, correlate with the modifications taking place in pathological series of all grades, and in

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geratologous and retrogressive forms of all kinds, however progressive they may be in certain characteristics. Geratologous forms, therefore, show that the development of hereditary retrogressive characters has been stimulated so as to take the place of the progressive, thus either partially or completely replacing them. The partial replacement is often accompanied by the early development of the hereditary, progressive characteristics.

Geratologous forms may, therefore, be the highest members of progressive series, or the terminal members of retrogressive series, and the stimulation of the development appears to take effect upon both progressive and retrogressive characteristics, thus producing at the same time and in the same shell, (1) the earlier or premature development of some of the progressive characteristics combined with geratologous characteristics, (2) the earlier development of geratologous characteristics and their fusion with larval characteristics which occasions the complete replacement of progressive characters and occurs only in the extreme forms of retrogressive series, and in parasites.

13. The law of concentration in development seems to express an invariable mode of action of heredity, in the earlier reproduction of hereditary characteristics of all kinds and under all conditions. In progressive series it acts upon healthy characteristics and appears to be an adaptation to favorable surroundings, and in retrogressive series upon pathological characteristics, and is probably an adaptation to unfavorable surroundings usually leading to the extinction of the series or type.

14. In following up series, it has been found that the development of ancestral forms is simple and direct; that of their more specialized descendants becomes gradually indirect, acquiring complicated, intermediate or larval stages, and that of the terminal retrogressive or geratologous and pathological forms becomes again more or less direct.

15. The introduction of adaptive larval stages into the history of individual development in any series appears to be due to the direct exciting action of the surroundings, and their absence or subsequent suppression to some physical agency, changes of habit or protection, or to pathological causes.

All of these causes must, however, be considered as similar in their effects upon the young, *i. e.*, direct stimulants producing excessively rapid development of the ancestral progressive charac-

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teristics, or of the retrogressive or primitive larval characteristics, inherited from the progressive forms.

16. The law of organic equivalence may be descriptively stated as follows, the action of physical changes takes effect upon an irritable organism, which necessarily responds to the external stimulants, by an internal reaction or effort. This action from within upon the parts of the organism modifies their hereditary forms by the production of new growths or changes which are, therefore, adapted or suitable to the conditions of the habitat, or the physiological equivalents of the physical agents and forces from which they directly or indirectly originated. In so far then as causes and habits are similar they produce representation or morphological equivalence in different series and in so far as they are different they produce the differentials which distinguish these series from each other.

Mya arenaria : its changes in Pliocene and Prehistoric Times.

By E. S. Morse of Salem, Mass.

[ABSTRACT.]

At a previous meeting of the Association, the author showed that the species of shells found in the Indian shellheaps along the coast of New England differed in their proportionate diameters from the same species living to-day. He pointed out, moreover, that species belonging to similar genera, in the shellheaps of Japan, had changed in precisely similar ways. It was important to find out, if possible, the cause of these changes. A comparison between the shells of two common species, found north and south of Cape Cod, gave indications that temperature was the inducing cause. The two species selected were *Mya arenaria* and *Venus mercenaria;* the former extremely variable, the latter very constant, in its characters. Specimens of these species had been collected in great numbers, both recent and ancient. The following are the indices of *Mya arenaria:*—

RECENT.		ANCIENT.	
South of Cape Cod, 61.42.	North of Cape Cod, 61.67.	South of Cape Cod, 62.	North of Cape Cod, 62.78.

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of *Venus mercenaria*:—

RECENT.		ANCIENT.	
South of Cape Cod, 81.01.	North of Cape Cod, 81.10.	South of Cape Cod, 81.51.	North of Cape Cod, 81.81.

Since the waters south of Cape Cod are much warmer than those north of Cape Cod, it was reasonable to suppose that these changes were due to temperature, and that the higher index of the ancient specimens found in the Indian deposits might indicate a colder climate. This supposition receives some support in the fact that a measurement of specimens found in the glacial clays about Portland, Maine, and on the Kennebec river in the same state, gave the high index of 66, and a number of Norwich and Red Crag fossils of *Mya*, which he had the opportunity of measuring at the British Museum, had an index of 64; recent *Mya* from South End, England, having the low index of 58.30.

It was interesting to observe, that measurements of *Mya* in Japan gave, for the southern form, an index of 61.10, and, of a more northern form, 62.50.

A NEW PLAN OF MUSEUM CASE. By EDWARD S. MORSE of Salem, Mass.

[ABSTRACT.]

THIS paper described a new plan of museum case devised and constructed and in practical use at the museum of the Peabody Academy of Science in Salem.

CONSCIOUS AUTOMATISM.[1] By C. P. HART of Wyoming, Ohio.

THE APPLICATION OF NITROUS OXIDES AND AIR TO PRODUCE ANÆSTHESIA; WITH CLINICS ON ANIMALS IN AN EXPERIMENTAL AIR-CHAMBER.[2] By E. P. HOWLAND of Washington, D. C.

[1] For a short notice of this paper see SCIENCE for Sept. 7, 1883.
[2] A notice of this paper is given in SCIENCE for Sept. 7, 1883.

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SECTION G.

HISTOLOGY AND MICROSCOPY.

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[Several meetings of Section G were held but papers were not read before the section.]

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SECTION H.

ANTHROPOLOGY.

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ADDRESS

BY

PROFESSOR OTIS T. MASON,

VICE-PRESIDENT, SECTION H.

THE SCOPE AND VALUE OF ANTHROPOLOGICAL STUDIES.

EVERYTHING that comes before the human mind has to pass through a process of weighing and measuring, and receives a valuation according to the thinker's standards of merit. In this critical spirit let us pass in review those studies called anthropological, in order to form some estimate of their value according to the measures commonly applied to various departments of learning.

Anthropology is the application of the instrumentalities and methods of natural history to the study of man. The anthropologist, in this sense, is not a dilettante philosopher, who inquires about old things because they are old, or into curious things while they are curious, omitting all the great movements and needs of society, and overloading the baggage-train of progress with trumpery picked up along the march. The practical spirit of our age demands that he ask what truth, or good, or beauty comes from such investigations, and how he can make them subservient to human weal.

As to the scope of anthropology, we may be instructed by the other sciences. The natural history of any species, say of the domestic horse, includes many inquiries, such as the time and place of its origin; its ancestry; its pristine size, appearance, and mode of living. We should afterwards inquire concerning the archæology or the palæontology of the Equidae, their embryology, anatomy, physiology, diseases, abnormalities, and external characteristics.

Mr. Romanes would have a chapter on the intelligence of the animal, viewing its nature and amount, supplementing the discussion with notes on the various ways in which the horse manifests its

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mind, its wills, emotions and opinions. Horses do not construct elaborate houses like the ants and the beavers; but the members of all species occupy their daily lives in some habitual industries by means of which they wear out the excess of muscle. Sir John Lubbock would lead us farther, and show us that horses go in droves, follow a leader, plan migrations, attacks, and defence, amuse themselves, enjoy one another's company, improve in appearance, intelligence, and usefulness by cultivation, — in a thousand ways show themselves to be social creatures. At last Mr. Mivart would insist that the horse has its habitude (ἕξις), its manner of action, its economy (œcology), and its members are affected in a characteristic manner by heat, light, moisture, winds, the kind and quality and abundance of food and drink, by beneficial or injurious animal neighbors, and by the vital, procreative, inheritable energy with which they are endowed. These and many other kindred inquiries concerning this homogeneous group would constitute the science of hippology.

The conscientious devotee to this science would frequently ask himself what practical good would result from all this expenditure of time, thought, and resources necessary to collect specimens and facts, and to formulate his science. Could they be employed on some subject more ennobling and profitable to himself, better calculated to inform, enrich, and beautify mankind?

Now, instead of horses, let us substitute the *genus homo*, laying aside all predilections; and, if possible, let us imagine the student of anthropology to belong to quite another genus than the subject of his research. He would have, in the fourteen hundred millions of human beings now living on the earth, and in the remains of their congeners slumbering in its bosom, perhaps the best defined group of animals. Calling them a genus or a species, as you like, they are so well hedged off from all other animal groups that not the least embarrassment has ever disturbed the naturalist in distinguishing the anthropos even from the anthropoid. No one was ever puzzled to tell, concerning any living thing, whether or not it was a human being. The earth has never yielded a bone concerning which the practical anatomist stood in doubt whether it had been once part of a human body.

Now, I take it for granted that any inquiry whatever which would be useful or entertaining respecting another species would be intensified in importance having man for its object. Indeed, there are

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few questions which naturalists are wont to propose to their groups which ought not to be carefully considered when we are studying man. Before entering upon the weighing process, therefore, it may make our task more easy if we consider the present scope of anthropology, and briefly pass in review some of the questions which are being propounded by anthropologists every day.

When did man first appear on earth, — at what time and in what geological horizon?

Have all the individuals of our race descended from a common human ancestry? in other words, are we monogenists, or polygenists?

Where was the birthplace of humanity?

What manner of creatures were the first men in specific characteristics, in size, aspect, intelligence, and social condition? and how did he get here?

To all such queries, Haeckel aptly gives the name of anthropogeny; therefore, in order to be anthropologists we must be anthropogenists.

Another set of questions relates to that stretch of time which lies between the pristine man, or the pristine condition, and the beginnings of recorded history.

Have we complete, irrefragable evidence that our race has progressed from a brute-like condition, in which it was devoid of all experience and appliances?

What application must we make of Professor Tylor's belief that civilization has progressed upward like a column of vapor, some parts advancing while other parts are being rolled downward, but the whole ascending and expanding?

Granting that there has been improvement, what paths have been pursued?

Speaking of our own peculiar province, what is the real import of such discoveries as those of Dr. Abbott and Professor Whitney in establishing the great antiquity and early rudeness of the American savage?

Who were the builders of the mounds, earthworks, and cliff-dwellings, and the stone structures of Middle America?

What were the functions of these various edifices?

What credence is to be given to the early historians of American culture?

Already we have our schools of interpretation, such as the Ban-

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croft school and the Morgan school. Where, among these opposing schemes, does the truth lie?

In the administration of this science, there is occupation for the greatest diversity of talent. The biologists of our time are entering into the minutest inspection of the life-history of each animal form. With enthusiasm the embryologists trace the modifications of structure as they succeed one another in the germ. Before their eyes the very play of creation is dimly shadowed, and organic structure built up. They pass their work on to the anatomists and physiologists. Now, the anthropologist must endeavor to comprehend the work of all in its synthesis. As Newton and Laplace grasped the unity and organization of the stellar world, as Humboldt gazed upon all created things as elements of the universal cosmos, as Darwin first conceived the consanguinity of all living beings and their mutual help or harm; so the anthropologist seeks to unite all that can be known respecting man into a comprehensive science, and to study the innumerable correlations which bind the most incongruous actions and thoughts together into harmony. May we gain help, in solving questions of human origin, by carefully observing the evolution of the embryo?

Does a knowledge of the life history of the individual furnish a clew to the life history of the species?

What does a comparison of the anatomy of man with that of the quadrumana say respecting the genealogy of the species?

What are the proper methods and instruments of anthropometry, — observing the growth of children; the dimensions, angles, and curves of the cranium; the diversity and size of the brain according to age and sex and race; the weight of the body; the color of the skin, hair, and eyes; the muscular movements; the development of faculties; longevity, fecundity, and plasticity under change of environment; and vigor? and what are the legitimate inferences to be drawn from such investigations?

Finally, by what devices can the multitudinous correlations of structure and function in the human body find expression in graphic methods?

Another set of observers must now be brought into this great laboratory. We have to deal with a group of animals in which intelligence has manifested itself to such a degree as to dominate all other functions. Teleological inquiries can be no longer ex-

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cluded. Hitherto the application of scientific methods to the mind has required that we should be satisfied with sensuous results of thought, and forbidden us to inquire into the nature of the mind itself. Now, we are met at the outset with this puzzling question: Shall consciousness, or introspection be admitted as an instrument of observation?

How are we to record its dicta? and how (to borrow a term from the astronomers) shall we eliminate the personal equation?

Or, if we are not in a position to admit introspection among our tools of observation, can we not invent some delicate apparatus by means of which the strength of feeling and the inmost thoughts may be known and measured?

Does the brain generate thought as the liver generates bile?

What can science tell us concerning the existence of a human soul, non-material, and not susceptible of measurement by the standards of well-known forces?

How does it come about that children inherit the traits, tendencies, and faculties of their progenitors?

By what routes does the mind pass on its way from infancy to maturity?

What use should be made of the multitudes of inquiries prosecuted with reference to the minds of animals, in the study of human reason?

The student of anthropology frequently finds himself in sympathy with Wordsworth, singing,—

> "Our birth is but a sleep and a forgetting;
> The soul that rises with us, our life's star,
> Hath had elsewhere its setting,
> And cometh from afar."

If, as Mr. Spencer says, that which we inherit represents the accumulated experiences of a thousand generations, is it also possible to retain the consciousness of those experiences? Will the sensitiveness of consciousness keep pace with the growth of knowledge, and obviate the necessity of laborious records? In which case we should have mental and spiritual atavism explained, and that universal sympathy felt by cultivated people for those standing on the lower steps of civilization.

Now, whatever thoughts any other creature than ourselves may have, and leaving out the possibility of mechanical mind-reading

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in the future, up to this time the only knowledge men have gained about one another's thoughts has been acquired from expression. The expression of thought is language. Dr. Hoffman finds language in rock paintings and carvings; Col. Mallery, in gestures; Mr. Thomas, in the Maya hieroglyphics; and the glossologists, in human utterance. Happily for us, they are a clever set, and well up in their craft. Let us hear some of the questions they are discussing:—

What are all the devices employed by living creatures to express their thoughts, emotions, and volitions?

Which took precedence in the origin of language, signs, or vocal utterances?

What is the explanation of the origin of language?

What light does language throw upon the origin of species?

Is the evolution of language a safe guide to the knowledge of the unfolding of the human mind?

By what lines have the forms of speech progressed?

How far is similarity of language an evidence of consanguinity among peoples?

Is there a genetic relationship between monosyllabism, polysynthetism, and inflection?

What credit must be given to the ear, and the invention of writing, in the conservation, and lines of progress, of language?

How should languages be classified?

Here we may leave the students of language, and take a new guide. Looking over the earth, we behold men divided into races or consanguineous groups, filled with race prejudices, and restricted by race capabilities.

What are those external and anatomical characteristics which have become transmissible by inheritance? When and how were they fixed? Are we to imagine, with Dr. Kollmann, that certain race forms were fixed far back in the past, just as the chemical elements were made irresolvable by a former state of matter?

Of these heritable marks, which is the best criterion of race,—the skull, the color of the skin, the texture of the hair, language, art, social organization, or mythology? or is it certain fixed correlations of these and other characteristics? If so, what are the laws of correlation and conservation in the races? Should the same set of structures be depended on in each race? How many races of men are there? and are these species, or varieties?

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In what manner should the question of race enter into the administration of politics, economy, education, and colonization?

It is impossible to say when this subject of race first became attractive to human minds. In the oldest histories and on very ancient monuments, are to be seen attempts to classify the families of mankind. In all the encyclopædias, under the word "ethnology" will be found the schemes of modern writers. But, since the commencement of our century, the subject has been taken out of the hands of individuals, and has engrossed the attention of societies. Manuals of instruction have guided the voyager and the traveller in recording the characteristics of races. In Stanford's Compendiums, based on von Hellwald, Prof. A. H. Keane has commenced a codification and synonymy of all the tribes of men. This he proposes to follow up with a biographical dictionary of tribes. The Bureau of ethnology has collated the names, priscan homes, migrations, and bibliography of all the North American Indian tribes; so that we are in a fair way to know something about the races of men, by proceeding from particulars to a general view.

Passing from man to his works, we are face to face with æsthetic and practical art as a unique study. All art relates to human desires for food, clothing, shelter, for activity in peace and war, for beauty, for social and spiritual happiness. Mr. Tylor has taught us to look upon art products as species that have had an evolution, a life history; and this was very much the plan of Gustav Klemm. This sort of study has captivated many anthropologists, and they are asking such questions as these:—

Admitting that the arts have been progressive, what have been the lines of their elaboration?

May we, by a process of elimination, trace backward the life history of each art, as would a patent attorney or a chancery lawyer?

At what degree of workmanship may we be sure that flakes of flint, gashed bones, and wrought wood, give evidence of human handicraft?

When does similarity of art-forms indicate social or commercial contact? when, consanguinity? and when, merely the same grade of culture?

Is degenerate art a facsimile of early, progressive art?

Is it allowable to fill up the gaps in the arts of any tribe by seemingly intermediate forms from other tribes?

Whence is the sense of beauty?

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The answers which we unconsciously give to these queries are the major premises of our arguments respecting the history of civilization.

By marriage in some of its forms, human beings are united into consanguineous groups, whose varied needs demand and produce many bonds of union, and widen the separation from other groups. With reference to each set of duties in the tribe, unwritten or written codes embody a system of ethics, regulating conduct in every particular. Farther on in their history, groups have relations of war and peace, and the absorption of homogeneous and heterogeneous peoples into a defined area gives rise to nationalities.

Were men ever herded together in promiscuity?

What were the earliest forms of social life?

What were the most primitive forms of marriage in groups?

Have all the tribes of men passed through the same systems of consanguinity and affinity?

Can the highest systems of altruistic ethics be explained by natural processes?

What are the most beneficial relations of labor to natural resources? and how have the present relations been brought about?

What is the history of the control of the body politic over the individual, and of the jurisdiction of corporations? and to what extent may individual freedom be controlled without discouraging private ambition?

What has been the life history of communism, crime, fashion, and politics?

Is it possible to regard and define facts in sociology by the terms of physical science?

Again, these human beings spend a great portion of their time acting and speaking as if other eyes and ears than those of mortals were cognizant of them. In the darkest nights, at the rising sun, throughout the day, at certain seasons of the year, this unseen world is invoked. In groves, in caverns, in estufas, or in costly temples, it is all the same: praises, petitions, and offerings confront the inscrutable power that can work men weal or woe.

How did man come to believe in the animation of things, fetiches, the wanderings of ghost-souls, spirits benevolent and malignant, the gods of classic mythology, and the Great Father of all?

What are the first conceptions of children respecting such things? and will these guide us aright to the childhood of faith?

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Has the history of mythology run parallel with the history of material and intellectual progress?

How may we divest ourselves of the personal equation, and learn the true psychology of savage worship?

Is Dr. Brinton right in applying the rules of interpretation adopted for Aryan mythology to American Indian myths, and in assuming that their crude stories are disguised deifications of the phenomena and powers of nature?

Finally, as men wander about the earth, and certain families are to be found chiefly in certain localities, so is it with races. Longevity, fecundity, and vigor are influenced by such causes as height above the sea-level, purity of the atmosphere, amount and distribution of heat, moisture, winds, fertility of the soil, and proximities, whether they be vegetal, animal, or human, whether they be beneficial or injurious.

By what subtle chemistries of the things around us, by what exposures in this terrestrial camera, come to pass the various hues of the skin and hair and eyes, the long skull and the short skull, the long face and the broad face, and the fixed compounds called natural characteristics?

By what processes of selection and adaptation has this cosmopolitan species come to occupy the whole earth, its genial climes, its frozen areas, and its fever-cursed tropics?

Is it possible to control these phenomena, or to adjust the human machine so as to anticipate and assist nature, to expedite natural selection and the survival of the fittest? or even to subdue nature, and decide for her what shall be the fittest to survive?

From this hasty survey of the scope of anthropology, we return to inquire what benefit the world derives from the cultivation of this science.

I answer, first, that every study is improved by study. All things become clearer to him who steadily fixes his gaze upon them. The sciences all began with vain speculations,—astronomy with astrology, chemistry with alchemy, geology with cosmogonies, biology with nature-worship, and theology with mythology. Long before the word "anthropology" was employed in its present acceptation, Alexander Pope wrote, "The proper study of mankind is man." But, millenniums before his day, mankind studied mankind by the light of their time. The study of man is no new thing, therefore. Now, since human thought has run, and will

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continue to run, in that direction, it becomes our privilege to rejoice that the stream has in these last days run wider and deeper and clearer. The proper study of mankind is the scientific study of man, the multiplication of rigorously exact observations, the collection of thousands of well-authenticated specimens, the classifications of both observations and specimens on rational bases, and the limitation of our conclusions to the extension of our premises. Some of my hearers have worked systematically and patiently for years at American archæology, and the anthropology of the modern Indians; and you rejoice with me to-day that our science has at last attained dignity and respect. With profound veneration I mention the names of Hildreth, Atwater, Stephens, Gibbs, Schoolcraft, Morton, Gallatin, Wyman, Squier, and Davis: with what buoyant hope they looked forward to this day, and with what exquisite pleasure must such living witnesses of the beginning as Horatio Hale, Col. Whittlesey, Dr. Jones, and Mr. Hempstead now contemplate the progress of solid work! The Smithsonian Institution will have to republish Squier and Davis, with many additions and corrections by Dr. Rau; the Bureau of Ethnology will antiquate Schoolcraft and Gallatin and Gibbs; Morton's and Wyman's work will be entirely superseded by that of the Peabody Museum and the Army Medical Museum. The Archæological Institute of America will throw new light upon the researches of Stephens; and Mr. H. H. Bancroft will make it entirely unnecessary to wade through thousands of pages of ancient Spanish literature. Therefore the first and most beneficial result of modern anthropology has been the direction of an immense amount of rambling and disorganized labor into systematic and rational employment. This clearing of rubbish, correction of misconceptions, cultivation of a modest spirit, willingness to abide the result, multiplication of materials, refinement of instruments, improvement of processes, in a study which thousands are determined to pursue, must strike every thinking person as a wonderful reformation.

Secondly, the value of a study must be estimated by its effects upon human weal. Farmers, miners, fishermen, lumbermen, mechanics, are slow to recognize their debts to the man of science. But who can estimate the millions of dollars saved by such studies as those of Packard, Riley, and Thomas, on the grasshopper, potato-beetle, and army and cotton worms, and the confidence engen-

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dered by the belief that a knowledge of the habits of these animals would lead to their conquest? It would take but a few moments to show that this argument applies with manifold force to the study of man himself.

It is not enough for the good physician to know the nature of remedies, or the use of knives and diagnostic apparatus. Sad will be his use of these if he has not familiarized himself with the structure of the human body in health and in disease, and, above all, if he has not made a correct diagnosis of his patient's case. Are not all the questions asked in the first part of this discourse, and many others agitated by anthropologists, connected with human welfare? Do they not relate to the body, mind, and speech of man, to the races of mankind, their arts, amusements, social needs, political organizations, religion, and dispersion over the earth? For instance, the French in Africa, the British in India, and our own citizens in malarious and fever-laden regions,— have they not learned from loss of treasure, ruined health, and the shadow of death, that there is a law of nature which cannot be transgressed with impunity?

It is the same with sociology and religion. The pages of history glow with the narratives of crusades against alleged wrongs, which were in reality campaigns against the sacred laws of nature. Social systems, which had required centuries to crystallize, have been shattered in the effort to bend them to some new order of things. Arts and industries planted in uncongenial soil, at great expense, have brought ruin upon their patrons, who had not studied the intricate laws of environment.

What a modification of temper, for instance, has been wrought among Indo-Germanic peoples by those studies in comparative philology which have led them by the hand back to their priscan home, and demonstrated, that though they may have aggregated into antagonistic nationalities, and fostered inimical industries, the same blood courses through their veins!

The better knowledge of race and race peculiarities has revolutionized and humanized the theories of aborigines. The doctrine of extermination, formerly thought to be the only legitimate result of colonization, has become as odious as it is illogical.

The inductive study of mind has hardly begun; but how much more successfully and rapidly will education and the development of the species progress when the teacher and the legislator can pro-

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ceed at once from diagnosis to safe prescription, when natural selection and human legislation shall coöperate in the more speedy survival of the fittest! The time seems to me to have arrived when our great anthropological societies and institutions should institute a systematic, coöperative study of psychology.

In a land where the archæologist may tally off most of his finds by savage implements in use at his very door, it seems like presumption to speak to you of the advantages of the most careful archæological methods. But there is a difference between the old and the new archæology. There are times in the settlement of a new country, when every man is his own carpenter, smith and physician. But how soon your energies have worked out of that! Now I speak only of professional archæology and its advantages. How many errors of his predecessors has Mr. Putnam alone corrected? We have all read with pleasure his recent correction of Dr. Hildreth's mistakes about iron in the mounds. It is so with your archæological collections: only those gathered in a scientific spirit will have any lasting value. But in the accumulation and preservation of such, you are the storers of force of the greatest value. You are recovering the scattered fragments of an ancient mosaic which will one day be reset, and its legend will be the lost history of prehistoric man.

The third benefit to which I will call your attention is the opportunity which the science affords for the exercise of every talent, even the highest. The difficulty of any problem depends upon the number or the degree of its unknown quantities. When facts were few, and the data of our science were beclouded with many sources of error, no wonder that men of logical minds left these investigations to those of a more imaginative disposition. Their crude, preliminary efforts have given place to organized work, directed by men of the greatest executive ability, assisted by skilful specialists, and endowed both by private munificence and by public appropriation. Not to go beyond the limits of our own country, we all point with pride to the Peabody Museum, the Archæological Institute of America, the American Antiquarian Society, the museums of New York city and of Philadelphia, the Smithsonian Institution, National Museum, Bureau of Ethnology, Army Medical Museum, and the Anthropological Society at Washington, the academies of Cincinnati, St. Louis, and Davenport, and the historical societies of many of our states, including the Minnesota collections.

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Now, the special merit of such great centralization of resources is that everybody can study something. It is possible for every craft and profession thus to prosecute its researches and to make its contributions. During the past winter, papers were read before the Anthropological Society at Washington by comparative anatomists, biologists, archæologists, geologists, physicians, paleographers, sign-linguists, philologists, patent-examiners, artists, statisticians, sociologists, clergymen, metaphysicians, and ethnographers. And this does not exhaust the scheme. Mothers, school-teachers, those in charge of the insane, of the criminal, and of the defective classes, lawyers, mechanics, musicians, philanthropists, legislators, may all contribute to this science some handiwork which will help to make the pile complete. To be still more personal, permit me to say to each one before me, that there is anthropological work which your peculiar occupation fits you to do better than any one else on earth. For example, a distinguished ornithologist, Mr. Henshaw, has recently identified all the birds in the well-known mound-pipes. An artist, Mr. Holmes, has succeeded in bringing order out of confusion in the shell ornaments of the mounds. A patent-examiner, Mr. Seely, traces backward aboriginal art. A general in the British army, Pitt-Rivers, worked out the history of the elaboration of the implements of war. An educator, Mr. Peckham, has recently given us the result of a laborious investigation on the growth of children. The geologists must interpret for us the significance of our discoveries in the drift. Where can I stop? I will boldly avow that the day of tyros is gone. There is a great multitude of collectors throughout our states who will have to go to school to Professor Putnam, or Dr. Rau, or Dr. Thomas, before they will have the faintest conception of the significance of their treasures.

The inevitable result of special research is generalization. Kepler, Newton, Count Rumford, Kirchhoff, Bunsen and Darwin, are names that stand for these processes in material science. To Herbert Spencer we are indebted for the first effort in this direction respecting human phenomena, and his work will be revised and corrected by those who will approach the task with better instruments and more reliable material.

In this heaving mass of humanity, returning into itself ever with vast gulf-streams and eddies, each actuated by its special

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forces, there is, after all, orderly motion. We discover that our little circle is part of a greater circle, and for a moment the mind is satisfied in the contemplation of this wider truth. Recovering, and renewing our investigation, the fact is reached, that this and its congeneric circles are part of a greater movement more complicated and perplexing. By the pursuit of this wider knowledge the intellect is strengthened, and thereby is brought about the natural selection of the mind. While many tire, or are unable to comprehend the situation, others press on, and grow strong by the effort.

The last advantage of which my time will allow me to speak, is the assistance which such studies render to philanthropy and legislation.

Standing on the deck of a steamer, and looking at the land left behind, we seem to be but a mile or two away. We are surprised with the information, that what seems so near is many miles distant. It is so with human history. In our childhood we believed that the first man walked the earth only a few centuries ago. All the events known to us then could easily have occurred in that brief period. The increase of knowledge expands the boundaries of time, and the origin of man is now lost in the mists of the past. Could anything fill our minds with greater love for our race than the magnificent struggle they have made in these millenniums? At the other end of the journey we were little better than brutes, and now we look out upon the cosmos as something reasonably comprehended.

If "pity for a horse o'er-driven" fills the heart of the poet, with what tenderness should we look upon the savage races, and remember that the whole family of man has stopped, some time or other, at that way-side inn! Each aberrant form, abnormality, criminal, dwarf and giant, shows the by-paths of human growth into which our life-stuff may have wandered. The arrow is the parent of the cannon-ball; the stone or bone spear-point, of the bayonet; the flint chip, of all edged tools; the cave-man, of the French *savant;* the hut, of the palace; the tattoo, of regalia; the gorget, of the crown jewels; the quipu and pictograph, of the printed book; promiscuous concubinage, of holy wedlock; the hunting-party of society; the clan, of the state; the fetich, of the pantheon; and universal animism, of universal causation. Instead of

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our ancestral belief in a tree with roots in the earth and branches in heaven, our tree has its roots in the past, and is ever putting forth leaves and flowers in a brighter present.

All sciences are retrospective. The astronomer, the physicist, the biologist, find the bases of their prophecies in the past history of the universe. The statesman, if he be wise, will imitate their example, and feel secure of his legislation for the future only so far as it is founded upon an intimate knowledge of the past.

The value of this study to philanthropy is easily shown. With what admiration do we read of the devotion of those missionaries who have suffered the loss of all things in their propagandist zeal. Science has her missionaries as well as religion, and the scientific study of peoples has notably modified the methods of the Christian missionary. The conviction that savage races are in possession of our family records, that they are our elder kindred, wrinkled and weatherbeaten mayhap, but yet worthy of our highest respect, has revolutionized men's thoughts and feelings respecting them. The Bureau of Ethnology has its missionaries among many of the tribes in our domain, no longer bent on their destruction, but treating them with the greatest consideration, in order to win their confidence, to get down to their level, to think their thoughts, to charm from them the sibylline secrets. It sounds something like the old Jesuit relations, to hear of Mr. Cushing at Zuñi, eating vile food, wearing savage costume, worshipping nature-gods, subjecting himself to long fasting and vigils, committing to memory dreary rituals, standing between disarmed Indians and their white enemies on every hand, in order to save their contributions to the early history of mankind. You will recall the fact, that an honorable senator more than a year ago offered, as an argument against sudden disruption of tribal affinities, an elaborate scheme of the Wyandotte confederacy. Max Müller says, "He who knows little of those who preceded him is likely to care little for those who come after. Life would be to him a chain of sand, while it ought to be a kind of electric chain that makes our hearts vibrate with the most ancient thoughts of the past as well as the most distant hope of the future."

In the study of this anthropo-cosmos, as in other studies, we are brought face to face with the inscrutable. In these voyages of discovery we have no right to expect that we shall ever find a passage to the ultimate truth. As with the child, so with the man;

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as with the individual, so with the race; as in the past, so in the present and the future,— the solution of one problem only prepares the way for many far more complicated. With all our sciences comes the consciousness of new ignorances. There is more known to be unknown now than when wise men knew that they did not understand many things well known to us. So will it ever be. Just about one hundred years ago, Peter Camper's measurements of the facial angle, with a few observations on height and weight, were thought to be all that anthropometry could furnish to the natural history of man. In 1881, Paul Broca laid down for the skull and the encephalon more than one hundred and fifty measurements; and the Germans go beyond that. Think you, the weighing and measuring will stop at these? We are just on the threshold of applying experienced training and instruments of precision to the study of man. Examine, if you please, the circulars for information issued by the old Paris ethnological society, Albert Gallatin, Lepsius, Max Müller, and the Smithsonian Institution, with those published for the Novara expedition, by the British Association, Kaltbrünner, Roberts, the new Paris society, or Major Powell, and you will have ocular evidence of the advance of anthropology.

But there is no Ultima Thule in science. No question propounded to nature will ever be answered. I can imagine the night of despair that would settle around any one of my hearers when he had reached the consciousness of having gathered the whole harvest of truth. On the other hand, I am sorry to hear any of our great thinkers uttering the words *ignoramus et ignorabimus* as a wail of despair. They should be to all the sweet voice of hope. They do not mean that we know nothing, or that we shall ever remain totally ignorant. Fresh, vigorous, buoyant, science feels itself to be on a pleasant journey, whose destination may remain unknown, but every mile of whose progress unfolds new vistas of beauty and variety in nature, each transcending the other.

I congratulate you, dear friends, that the American Association has delegated to you such an important trust. The illustrious names to be found among our members and fellows are a sufficient guaranty that you have lighted your torches, and that our science will not be a laggard in this grand march. Professor Henry said, in 1859, "The grand statement cannot be too often repeated, that each branch of knowledge is connected with every other, and that no light can be gained in regard to one which is not reflected upon

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all" (Smith. Rep., 1859, p. 15). We may go farther, and say, that, whenever any marked generalization is made in any science, all other sciences proceed at once to put themselves in line with the new order. It is the duty of the anthropologists, therefore, not only to rejoice in the growing light of chemistry and biology, but, quickened by their warmth, to put forth new life and vigor, and to apply to their investigations the most refined instrumentalities and the most subtile thought; believing with Lord Lytton "that man is a subject of far nobler contemplation, of far more glowing hope, of a far purer and loftier vein of sentiment, than all the 'floods and fells' in the universe."

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PAPERS READ.

A Classification of the Sciences. By J. W. Powell of Washington, D. C.

Vestiges of Glacial Man in Central Minnesota. By Franc E. Babbitt of Little Falls, Morrison county, Minn.

[ABSTRACT.]

During the open winter of 1878–79 and the season immediately succeeding it, the writer of this paper gave much time to searches upon certain terrace surfaces of the Mississippi river, for palæolithic remains supposed to exist at the locality under examination. Rudely worked quartzes had previously been discovered here by the state geologist of Minnesota, Prof. N. H. Winchell, by whom they had been described and figured in the state geological report for 1877. The field thus explored lies in central Minnesota, about 100 miles northwest of St. Paul, and within the township and village of Little Falls, Morrison county.

The find reported by Prof. Winchell consists of chipped objects of a class generally ascribed to what is called the rude stone age. Of these many appear to be mere refuse, while others are regarded as finished and unfinished implements. The Winchell specimens have been assigned, upon geological ground, to a prehistoric era antedating that of the mound-building races, and reaching back to a time when the drift material of the terrace-plain was just receiving its final superficial deposit. It is found that, at intervals, the surface soil of the terrace contains these quartzes to a depth of, not unfrequently, three or four feet.

The lowest and newest formation at this place is the present flood-plain of the river. It is still in process of deposition, being yet subject to partial overflows at periods of exceptionally high

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water. In that portion of the town of Little Falls situated east of the Mississippi, this bottomland is limited on the east by a high ancient river-terrace, which has here an average elevation of about 25 feet above the river at the head of its rapids called Little Falls (of seven feet descent) and has a width ranging from less than a mile up to several miles. This older terrace, like the present flood-plain, has been spread out by the immediate action of water. It consists of stratified gravel and sand, and forms, according to Mr. Warren Upham, assistant on the state geological survey, a part of the modified drift which was deposited at the close of the last glacial epoch of the great ice age, by the floods that descended along this valley from the melting and retreating ice-sheet, being derived from the portion of the drift which had been contained in the lower part of the ice.

While occupied in examining the river bank at Little Falls in quest of wrought quartzes, one day during the season of 1879, I had occasion to ascend a slope lying between the new flood-plain and the older terrace, by a path leading through a sort of gap or notch in the latter (310 rods, very nearly, or almost one mile, north of the east-west road by Vasaly's hotel; 10 rods west of the road to Belle Prairie; and 38 rods from the river). For the purpose of more pronounced distinction I will denominate the lower and higher levels under consideration as bottomland and terrace-plain, respectively. It seemed that at some past period a cut had been effected here by drainage, and that the washout thus formed had afterward been deepened by being used now and then as a wagon track. In this notch I discovered the soil to be thickly strewn with pieces of sharp, opaque quartz. These were commonly of a white color, and ranged in size from minute fragments to bits as large as a man's hand, and in some instances even larger. There were many hundreds of these chips visible, scattered over an area the width of the wagon road, and ten or fifteen yards in length. They were conspicuously unwaterworn, and likewise mostly unweathered, though occasionally a bit was picked up having some one of its surfaces weathered, while fractured or wrought faces, appearing upon other parts of it, looked as fresh as if the work of yesterday. On the other hand, the mass of stone rubbish upon and among which the quartzes were strewn is much waterworn, many of the pieces being well rounded, while none of them are wholly angular.

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By continued observations at this locality, I found that many of these quartz chips were brought to light at every succeeding freshet of the season, being washed out of the sand by descending drainage. Their immense and continually increasing numbers seemed to warrant the belief that they had resulted from systematic operations of some sort, once conducted, for unknown purposes, upon this particular spot. A portion of the studied specimens subsequently yielded evidence of having received shape from human hands, and therefore it was assumed provisionally that the site of exposure represented a prehistoric workshop.

Prolonged investigation ensued; and investigation established the hitherto unsuspected fact that no quartz chips or fragments were enclosed in the upper part of the gravel and sand terrace at the notch, nor within a considerable distance at either hand, though they were sought with careful scrutiny. Since November, 1879, at which time the studies here recorded were in active progress, the wagon track before mentioned has been converted to public use as a highway, and has been repeatedly graded in consequence. As might be conjectured, the ground has been much dug over, the stratum being at present almost broken up and destroyed, if not wholly so. During three successive seasons the road has been in a state of almost constant repair, owing to its being continually used for the heavy work of dragging logs from the bottomland to the top of the terrace-plain. From this cause the topography of the spot has been materially modified of late, while quartz chips from the bed of the notch, as likewise from the stratum at first undiscovered, have been much mixed up with the surrounding soil. Primarily, however, as I permit myself to reiterate by way of emphasis, I could not, with a great deal of painstaking, find any superficially inhumed quartz fragments at the point of observation, although, as shown by Prof. Winchell, and as I have myself had abundant occasion to observe, such quartz-chippings occur in the surface stratum of the terrace-plain at various isolated localities in the vicinity. So far as I have been able to judge, these appear in groups which are surrounded by considerable areas containing none. The absence of chips above a plane a foot or two superior to that of their bed at the bottom of the notch, was, of course, a marked circumstance, since they should have occurred plentifully in the superficial stratum, had the notch chips been of the same age with Prof. Winchell's find.

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Ultimately it was ascertained that the notch quartzes had dropped to the level at which they were seen from a thin layer of them once lying from ten inches to two feet above it, and subsequently broken up through the wearing away of the sand underneath by drainage. This layer or stratum was still intact on the north and south and partially so on the east, in which direction it had, however, at certain points, suffered some displacement by wagoning. It extended in a nearly horizontal plane into the terrace, in the sloping edge of which the notch, opening into its west bank and truncated at its edge, is cut. The eroded gap in this stratum was oblong in shape. Its outlines, lying within the slopes of the notch, were masked from ordinary observation by thin slides of soil from higher surfaces. Both the inferior and superior planes of the quartz-bearing stratum were sharply defined. Its horizontal boundaries had not been distinctly reached in any direction at the time of excavating for the road, and they have since been partially, and perhaps wholly, obliterated by the grading and digging over to which the spot has been subjected. At the west, the stratum reached to the limit of the terrace, which is bounded by a steep slope. The quartz-bearing layer averaged a few inches only in thickness, varying a little as the included pieces happened to be of smaller or larger size. The contents were commonly closely compacted, so much so that one might sometimes extract hundreds of fragments, many of them very small ones, of course, from an area of considerably less than a square yard.

The quartz bed, so far as examined, rested upon a few inches of sandy soil, which passed downward into a coarse, waterworn gravel, immediately overlying till. Above the quartz chips, stratified gravel and sand extend up to the surface of the terrace. The pebbles of the gravel lying directly on the quartz-bearing stratum, were small and well rounded, and were noticeably less angular than those of the gravel below. The stratum of quartz chips lay at a level some twelve or fifteen feet lower than the plane of the terrace-top.

These observations show that the quartz chips were spread originally upon an ancient surface that has been since covered deeply by the modified drifts which forms the terrace. It will be remembered that the quartz chips and implements discovered by Prof. Winchell in this vicinity are contained in the upper stratum of the terrace-plain; but the notch quartzes do not occur at the

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terrace-top, and cannot have been derived from it, but are confined strictly to a single stratum of the lower gravels closely overlying the till. Hence the two sets of objects cannot be synchronous, though they may have been produced by the same race at different stages of its existence. The notch quartzes must, of course, be older than those described by Prof. Winchell, by at least the lapse of time required for the deposition of the twelve or fifteen feet of modified drift forming the upper part of the terrace-plain, above the quartz-bearing stratum.

Some of the quartz chips in the stratum have been distinctly arranged upon the floor of deposit in little rudely assorted heaps or groups of objects of particular types. During the summer of 1879, upon the day when the existence of the quartz-bearing stratum was first definitely settled, I collected from it about half a peck of the quartz pieces. Among these appeared a group of specimens numbering a dozen or more, which belonged to a type unknown to me at the time. The most finished implements of this shape have delicate fragile edges, formed by a single thin leaf of the quartz prolonged beyond the mass of the object in a series of irregular minute notches. Further exploration of the site continued up to the present time, has, so far as observed, failed to unearth a single specimen of the same class elsewhere. Whether we assume these small articles to be natural, or artificial productions, or whether, again, we hold them to be natural forms more or less modified by the hand of man, there is no escaping the conviction that the grouping together of these and other like objects is wholly the work of intention. Previous to the discovery here narrated, I had several times chanced upon bunches of specimens of a common figure lying in separate groups in the sand at the edge of the stratum, apparently just washed out of it. Similar assortments of specimens, having a common form, have been brought to light in many portions of the stratum. Indeed, wherever I unearthed any individual object of comparatively large size, it was very often, though by no means invariably, found to be associated closely with others of a kindred type. I have exhumed from this place groups of axe-like quartzes, of rasping stones, of broken implements, of long prong-shaped objects perhaps utilized by primitive man as weapons, and of other rudely fashioned pieces. There were also quartz-fragments of several forms, which were scattered through the whole deposit, such as hammer-stones

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of different shapes, sharp pieces adaptable as cutting blades, and a great many sharp and long splinters.

These quartzes include many distinct varieties of the mineral, showing marked differences of opaqueness, color, hardness, manner of fracture and admixture of foreign matter. The greater part of them were apparently taken from the quartz-bearing slate in the vicinity. The mode of occurrence of the more unusual specimens is noteworthy and leads to the inference that they may have been fashioned from drift bowlders, numbers of them, indeed, having been evidently formed of waterworn pebbles. Objects shaped from some special variety of quartz not unfrequently present themselves in loose groups, varying in number from two or three to a dozen pieces. Thus a single specimen of a tinted, mottled, or otherwise distinguishable quality, has quite commonly been found associated with a few others of the same kind of quartz. When one of these is of comparatively large size, the remaining objects are, in many cases, proportionally small; as if they were chips from the largest.

A collection of the quartz objects found in the notch at Little Falls was exhibited before the Section, and many of the specimens were considered to be unquestionably the work of man. The collection was afterwards given to the Peabody Museum of American Archæology, and is referred to in the report of the Curator of the Museum for 1883.

Abnormal Human Skulls from stone-graves in Tennessee.

By F. W. Putnam of Cambridge, Mass.

[ABSTRACT.]

The photographs exhibited are views of a brachycephalic[1] skull, taken by me, in 1882, from a stone-grave on Dr. W. H. Jarman's farm near Brentwood, Tennessee. It is that of an adult, probably a man. The coronal and sagittal sutures are simple and all the sutures except the basilar are open. The left parietal is divided into two quadrilateral pieces by a transverse serrated suture, crossing the bone at one-third its height above the squamous suture. The transverse arc formed by the divided left parietal exceeds that of the normal right parietal by one-fourth. The

[1] This skull is No. 27205 of Peabody Museum, and has a breadth index of 976.

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longitudinal arc of the left parietal is also greater than that of the right parietal, and the left side of the skull bulges upwards and outwards. The abnormal suture encloses a pair of Wormian bones (size 3 of Broca's scale). They occur close together a little back of the centre.

The back of the skull is much flattened and the sutures of this region are crowded with supernumerary bones. In the right parietal-mastoid are three adjoining Wormian bones of medium size, and there is another of medium size in the right masto-occipital suture. A long oval Wormian bone extends from these along the lambdoid suture to near the median line, where it interlocks with a very long Wormian bone which fills the left half of the lambdoid suture to the upper posterior angle of the lower segment of the left parietal. Between this lower piece of the left parietal and the occipital bone are three large and two medium-sized Wormian bones. A large Wormian bone lies to the right of the lambda between the largest Wormian bone and the right parietal. This cranium has in all several minute, fourteen medium-sized, four large, and two unusually large Wormian bones.

All the teeth are present, and normal, except the two lateral incisors of the upper jaw, which, apparently, were never developed. The gap separating the central incisors and canines is scarcely broader than that between the two central incisors.

Another brachycephalic adult male skull[2] found in 1877 in a burial mound[3] on Mr. Wilkinson's farm, nine miles from Nashville, has the abnormal suture dividing the left parietal. This skull has simple sutures which are open except near the pterion. The extra suture springs from the left arm of the lambdoid suture at the junction of its middle and lower third, and passes nearly horizontally half-way across the parietal. It is then deflected and runs towards the pterion. Posteriorly this suture is finely dentated but anteriorly it becomes simple and is so much obliterated that it is impossible to trace its entire course. A large Wormian bone occurs in the left arm of the lambdoid suture at and above the origin of this anomalous suture.

The posterior inferior angle of the right parietal of this skull is also developed from a separate centre. It is united with the main portion of the bone by a simple suture running from the squamous

[2] No. 12797 Peabody Museum. Breadth index, 1012.
[3] This mound contained a number of stone-graves placed in several tiers.

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suture almost horizontally till it approaches the lambdoid when it turns downward and enters this suture at a point opposite the middle of the squamous border of the intercalated bone.

Division of the parietal is a rare anomaly. Of more than two thousand skulls which have passed through my hands, the two here described are the only ones that I recall as having this anomaly of the parietal. Miss C. A. Studley, an assistant in the Peabody Museum, has looked up the literature of the subject for me and finds that Grüber describes one case and records eleven others as known up to 1870, and that in Flower's Osteological Catalogue two crania are mentioned as having a supra-squamosal bone.

A new Stand for Skulls, made by Edward E. Chick, Assistant in the Peabody Museum of American Archæology and Ethnology. By F. W. Putnam of Cambridge, Mass.

[ABSTRACT.]

Various methods have been adopted for mounting human crania for exhibition in museum cases, all of which, so far as I have seen, are defective in some one or more particulars. The objects to be secured are the proper exhibition of a skull without injury, and mounting it in a natural position in such a way that it may be placed in the case so as to exhibit it either from the front, back or side, as may be desirable for comparison with other skulls in the case. Further, it is essential that it may be easily removed for special study, and replaced without much trouble when done with for the time being. It is also of the greatest importance to avoid any pressure on the margin of the foranum magnum, on the bones of the palate, or base of the skull, which in crania of considerable antiquity are generally very friable. Another essential thing is to be able to suspend the under jaw in place without making holes for the passage of wires.

After many trials and modifications, all these points have been secured by my assistant, Mr. Chick, and the stand now in use at the Peabody Museum consists of a round wooden base, in the centre of which is screwed a brass rod from three to four inches high, on which a screw thread is cut. On this upright rod a cir-

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cular brass plate, with a screw thread, is placed at any desired height. On opposite sides of this plate two brass wires are screwed and so curved as to support the occipital portion of the skull. These wires can be easily bent, and their curve can be changed by the fingers so as to fit any particular skull. A third wire is screwed into the front of the plate and projects from three to four inches, ending in a ring. This rod can be easily bent so as to obtain a proper bearing for the ring on some firm part of the palate, or, if need be, when the palate is broken, on the inner margin of the teeth or any other firm point. The pliability of this rod also secures the tilting of the skull into a natural position as it rests on the occipital wires and is steadied by the central rod which passes into the foranum magnum. From the forward rod hangs a small spiral wire holding a transverse piece of wire with the points turned up so as to clasp the under jaw on each side, the strength of the spiral spring being sufficient to hold the jaw in its place and clear of the shelf.

On the stand itself the label can be placed and the skull thus mounted can be turned about at a slight touch as it stands, swinging free from the shelf. In this way all ordinary observations can be made without running any risk in handling the skull. This stand may be adapted also for mounting fragile and broken crania and even portions of crania in a natural position.

THE CHARNAY COLLECTION AT WASHINGTON. By O. T. MASON of Washington, D. C.

[ABSTRACT.]

THE collection referred to contains the material obtained by the Charnay expedition to Mexico and Central America. The expenses were defrayed jointly by Mr. Pierre Lorillard and the French government. The success of the expedition showed how a gentleman of fortune might render valuable service to the cause of science, although not specially conversant with scientific lore.

In this undertaking the point was gained by obtaining, as far as possible, casts (in plaster strengthened with tow) of the objects of antiquity. By means of these casts, the drawings of this and

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other exploring expeditions can be verified and corrected. Great success was attained, as to casts from little-known and almost inaccessible ruins; and many new objects of beauty and curiosity were brought to light, among them a large number of interesting reliefs and statues. The casts will be preserved in the Museum at Washington, and in duplicate at the Trocadéro Museum at Paris. Numerous photographs and drawings were obtained; and measures have been taken, under the auspices of the Smithsonian Institution, to reproduce several of the more important ruins by correctly arranging the casts in position with suitable accessories. Good success has already been attained, both in making restorations of ancient temples and other ruins, and in correcting recorded measurements and drawings. A full account of the restorations will appear in Professor Baird's annual report for 1883.

OSAGE WAR CUSTOMS. BY J. O. DORSEY of Washington, D. C.

[ABSTRACT.[1]]

BY means of an illustration the preparations were shown which the tribe makes for a war-march, the order followed being that of gentes or clans in the tribe. The paper described the tactics by which the Osages camp in a circle, the war-men on one side, and the non-combatants on the other. The road travelled by the Indians forms the line down through the centre of the camp, and the division-line. The great war-tent is placed with the door to the west, the place of honor being at the east. The author detailed at considerable length, with the aid of illustrations, the method of selecting the forces, and the ceremonials preparatory to war, the decoration of the Cheezho peacemaker, the form of the dance around the village, the nature of the bravery dance, and the order of march from home by twos four abreast. Marriage ceremonials and funeral rites were also described and explained in detail. The author accompanied the paper with drawings illustrating the grouping of participants in ceremonies.

[1] This paper has been printed in full in the American Naturalist, for Feb., 1884.

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Observations on the laws and privileges of the Gens in Indian Society. By Miss Alice C. Fletcher of New York, N. Y.

[ABSTRACT.]

An elucidation of the customs and circumstances under which the gens system in Indian tribes surpersedes parental ties was the chief feature of this paper. The author had an excellent opportunity for observations of this character, during the work of placing the Omaha tribe of Indians upon their lands in severalty, and while adjusting the line of descent and inheritance according to our laws.

A child who has lost its father or mother is considered an orphan. Its particular place is gone, and it passes into the gens. When the father dies, leaving offspring, the mother loses all maternal rights. Each child, unless of very tender age, will be separated from the mother, and will go into the family of some one of the father's relatives. It may thereafter be claimed as his own child by the male head of the family to which it has been allotted. This separation of her children from a widow is permanent. She usually marries again, and in that event is not burdened with her offspring by a former husband; but if she should remain unmarried, she would be expected to work for the family that has adopted her children, rather than for the children themselves. If she dies when her children are young, it is probable, that, at maturity, they will have forgotten even her name.

The women are not wanting in affection for the children of whom they are bereft; but the separation is looked upon as a matter of course, none of the interested parties regarding it as a grievance, or even a hardship. It surprises an Indian to have the propriety of this arrangement questioned. No point of our law of inheritance is so difficult for him to understand as that which binds together the child and the surviving parent.

Young men whose mothers are of the same gens are accounted brothers to each other, and the brothers of the mothers are uncles. Between these uncles and the nephews and nieces there is an easy familiarity, not unlike that of parents and children.

The author has observed a decided lack of family likeness among Indians. This observation applies, however, to entire families,

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which include cousins, aunts and uncles; a striking resemblance between parents and children being not unusual.

The Indian may be "the stoic of the woods;" but he is neither averse to pleasantry, nor deficient in sensitiveness of a certain kind. He delights in chaffing his fellow Indian; and dreads, more than aught else, being made a laughing-stock.

Symbolic Earth Formations of the Winnebagoes. By Miss Alice C. Fletcher of New York, N. Y.

[ABSTRACT.]

Having mentioned in previous papers[1] the symbolic earth formations which appear in every religious ceremony I have witnessed among the Sioux, I will call attention to similar structures noticed among the Winnebagoes.

This tribe is admitted to be one of the older branches of the great family to which it belongs. Not only is this antiquity shown by the language, but by finding many of the religious ceremonies of different tribes referred to the Winnebagoes, and Winnebago words discovered in the religious ritual of other tribes.

The Winnebago tent used for sacred dances is long and narrow; not more than twenty feet wide and varying from fifty to one hundred feet long.

In the Buffalo dance which is given four times in the month of May and early June, the dancers are four men and a large number of women. As the dancers enter, each woman brings in a handful of fine earth and in this way two mounds are raised in the centre at the east, that is between the eastern entrance and the fire, which is about fifteen feet from the eastern entrance. The mounds thus formed are truncated cones. An old man said to me "That is the way all mounds were built; that is why we build so for the buffalo."

The mounds were about four inches high and not far from eighteen inches in diameter. On the top of the two mounds were placed the head-gear worn by the men, the claws, tails and other articles used by the four leaders, or male dancers.

The men imitate the buffalo in his wild tramping and roaring, and dance with great vigor. They are followed by a long line of

[1] Sixteenth Report of the Peabody Museum of Arch. and Ethn., Cambridge, Mass.

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gayly-decked women in single file. Each woman as she dances keeps her feet nearly straight and heels close together, and the body is propelled forward by a series of jerks which jars the whole frame, but the general effect on the long, closely packed line is that of the undulating appearance of a vast herd moving.

The women dance with their eyes turned to the ground, and with their hands hanging closely in front, palms next the person. The track left by their feet is very pretty, being like a close-leaved vine. It is astonishing to notice how each woman can hop into her predecessor's track. Water is partaken of and the entire dance is clearly indicative of the prayer for increase and plenty of buffalo.

The two mounds remind one of larger structures and suggest many speculations, particularly when taken in connection with the manner of their building.

In the great mystery lodge, whence so many of the sacred societies among other tribes professedly take their rise and inspiration, the fire is at the east, and is made by placing four sticks meeting in the centre and the other ends pointing to the four points of the compass. Just at that part of the initiation of the candidate when he is to fall dead to the old life, be covered as with a pall, and then be raised to the new life, the remains of the four fire sticks are taken away and the ashes raised in a sharp conical mound; again suggesting hints of a peculiar past.

Upon the bluffs of the Missouri, on a promontory overlooking a stretch of landscape thirty miles in every direction, is a little depression cut in the ground, circular in form, with an elongated end at the east. The depression is one foot in diameter and about six inches deep. Placing my compass in the centre, the long end or entrance was found to be exactly to the east.

To the south of this sacred spot, for it is cleared and cleaned by secret hands every year, stood a large cedar tree, now partly blown down. This was the sacred tree on which miraculous impersonation of visions lit; and here the spirits tarried as they passed from one resting place to another going over the country. About every fifty miles there is one of these strange, supernatural resting places.

The history of the earth symbols is not yet clearly understood, and it is to be hoped that students will carefully observe these strange relics of customs which were once, perhaps, on a grander scale.

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Life among the Mohawks in the Catholic Missions of Quebec Province. By Mrs. Erminnie A. Smith of Jersey City, N. J.

[ABSTRACT.]

The almost entire isolation, during more than two hundred years, of these Mohawks from the other Iroquois tribes, renders a study of their dialect, peculiar customs, remarkable habits of industry and prosperity, subjects both interesting and important. The old mission house and church at Caughnawager contain the old Jesuit library, portraits of Charlevoix and Lafitau, various presents from crowned heads in Europe, a portrait of the only aboriginal saint, Ta-gah-qui-ta,—all objects of great interest. The archives of this old church and of the seminary of the Sulpicians at the Lake of the two Mountains are the repositories of old manuscript sermons, dictionaries, grammars, catechisms, etc., some dating back over two hundred years. The dictionary of the Rev. Father Marcoux is one of the most valuable and well preserved of those extant in the Mohawk dialect, not excepting Bruges, etc. Interesting examples of its contents are the names of the months, days of the week, etc. The day is past when it could again be compiled, as the industries of these people are bringing them into so close contact with French and English that their own language is rapidly falling into disuse. The beadwork of the women is sold from Maine to San Francisco. Over ten thousand pounds of beads are kept constantly on hand in the wholesale store of one of the chiefs to supply the never-ceasing demand. The stone quarries are also worked by the chiefs, their owners, and are a great source of profit. But the principal occupation of these Indians is rafting upon the St. Lawrence. Few of the old customs are preserved, although still nominally presided over by chiefs. The clans here outnumber the other branches of the Mohawks by four, the Lark, Rock, Calumet and Dove.

The old wampum belts are still preserved.

The changes which a language can undergo, during a separation of portions of its people for only two centuries, are illustrated by the table prepared of words in use at Caughnawager, and their synonymes in use among the Mohawks of Upper Canada. This list proves conclusively that dialectical differences can be brought about in a comparatively short space of time. The old aborigi-

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nal folk-lore stories have been superseded through the influence of the church by those of a religious character, such as the Legend of the old Church bell, etc.

The other mission referred to was removed in 1717 from Montreal to the Lake of the two Mountains, where the Order of Sulpicians have a grant of seventeen miles of land and provide for the moral and religious education of the Indians of their mission. A convent of the *Congregation de Notre Dame* has charge of the school for Indian girls.

From these archives a large number of titles were secured for the bibliographer of Indian linguistics at Washington.

STUDIES IN THE IROQUOIS CONCERNING THE VERB TO BE AND ITS SUBSTITUTES. By MRS. ERMINNIE A. SMITH of Jersey City, N. J.

[ABSTRACT.]

THE general reader upon the structure of Indian languages and the student who would attempt their investigation are sure to encounter as a conceded fact, established by the pioneers in Indianology that "the verb 'to be' does not exist in these languages."

And when we remember that it is in all languages made up of several radically differing roots the above admission would seem correct.

Notwithstanding this, the conscientious student is staggered at encountering first in adjectives, and later in many verbs and in the dissection of nearly all nouns, the fact that in their translation into English the use of the verb "to be" is imperative.

Among the distinguished philologists who have expressed themselves upon this subject we mention Major Powell who in his "Introduction to the study of Indian languages" says "In most Indian languages there is no verb "to be" used as predicant or copula." Dr. Riggs in reference to Dakota verbs also remarks "We cannot now say more concerning verbs unless we add this interesting fact that as is the case in other American languages there is no pure substantive verb in Dakota.

We can express the idea of existence in connection with ideas of

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place or condition, but not alone. Rev. Father Cuoq in his "Lexique de la langue Iroquoise" gives as the translation of the verb "*etre*, to be," the Mohawk verb ke-nă'-ke-re which means literally *I inhabit* and is generally applied to animals.

The unknown author of the old Onondaga Dictionary translates it as Ki'-te-roñ which literally translated is, *I am seated, I am placed, etc.*

Father Marcoux, who is undoubtedly the best authority upon any of the dialects of the Iroquois says, "The verb *to be*, which according to Levizaz is the only true verb, does not exist isolated in this language, but is found contained in nouns, pronouns, and adjectives. It is expressed in different manners when it signifies the *essentiam simpliem in loco.*"

It is rendered thus:—

1. Ke-nă'-ke-re, *I inhabit;* applied to animals, fish, birds, and snakes it is translated by "to swarm," "to be plentiful," as *a.* Ok'-si hă-nă'-ke-re ne' Ră-weñ-ni'-yo; literally, *always he inhabits the God free.* "From the beginning God is." *b.* Keñ-tcă-nă'-ke-re, *fish swarm*, are plentiful.

2. Koñ'-he^e, *I live*, I survive.

3. Ki'-te-roñ, *I abide*, dwell, am seated, am placed; -ta-koñ a sufflx and shows the cause of, means of, "dwelling, etc."

4. Kă'-yeñ, *it lies*, as kă-ni kă-nă-tă-yeñ, *where it-lies-the-city?*

5. I-kă'-le, *it is present.*

6. Yă-oñ-te, *it has;* said of conditions arising from original design or purpose as Kă-nă'-toñ-te, *it has a room* (affixed to it); ră-yă'-toñ-te, *he has a body.*

7. Kă'-te, *it projects*, is arrayed; as Kă-roñ'-hyă-te, *a cloud projects*, is visible; in this it literally represents *ex-ist.*

8. Kă-hnyo'-te (a compound word, from "7th" and o-hnyo, *a bow*), literally *a bow is present, i. e.*, the rainbow.

9. A-koñ'-hă, *I, alone.*

10. Te-kyă'-se, *I am doubled*, twice myself.

11. Keñ, or ne'-keñ, *that*, or that which.

12. I-wăt, *it is in*, it lies in; sañ-ni-wăt, *how much is in?*

13. Wă'-toñs, *it becomes*, turns into—?

14. A-kwă'-weñ, *ours.*

15. Yo-weñh'-te, *it is joined*, is joined with $\left.\begin{matrix}\text{are}\\\text{is}\end{matrix}\right\}$ many.

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16. Yă'-oh it, *is in solution*, is in; as, Kă-hne'-ko^c, *water is in*, *i. e.*, there are springs; Kă-ris'-to^c, *iron is in*, iron-mine; Yo-hwis'-to^c, *a silver*, money-mine.

The Rev. Father LaCombe in his "Dictionnaire de la langue Crise," remarks in his introduction, "There are no auxiliaries *to be* and *to have* as in French, but they are replaced by post-fixes, which have the same value in making the verb."

As an interesting study, however, the following table was prepared containing mainly adjectives which in their conjugations are said to include the verb *to be*, and which are untranslatable into English without its use. This was for the purpose of separating from the *conjugated adjective* any particle representing the verb *to be*.

Hoñ'-tci, black.
K-hoñ'-tci, I am black.
S-hoñ'-tci, thou art black.
Ră-hoñ'-tci, he is black.
Yă-hoñ-tci, she is black.
Kă-hoñ'-tci, it is black.

In this conjugation, if a separable verb *to be* exists, it must be sought for in the pronouns often represented by a single letter.

The first person, present tense, of another conjugation, is as follows:

Wă-e'-tsă-nit, courageous.
Wă-a-te-wek, affable.
Wă-i-tent, poor.
} adjectives pure.

Conjugated.

Wă-ke'-tsa^c-nit, I am courageous.
Wă-kă'-te-wek, I am affable.
Wă-ki'-teñt, I am poor.
"K" in this representing "I am."

If these languages were as well understood as the Latin, might they not possibly be found to contain a letter representing both verb and pronoun, as in the imperative of the verb io, *I go* (Latin) where "i" represents go thou?

The above examples are given to show that the *present tense* of the verb *to be*, if such exists, must be contained with the pronoun in the letter "K."

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Pushing these studies farther, if one would wish to fill out the other tenses the *tense endings* must be suffixed. Taking "hăk" (the sign of past time) and suffixing it to "K" we have K-hăk′, *I was.*

But one encounters forms which must be considered irregular as in Kuñ-kwä^{e}-stă-kwăs-nu′-hä^{e}. *I-was-a-good-person.* In this example, K-nu^{e}-hä^{e} would represent *I was*, and so on through other tenses which forms however, standing thus connected, would be quite meaningless.

A study of these tense endings is at least not useless for they teach most conclusively that these languages are very old, and show that their verbs have incorporated as tense signs highly abbreviated words whose root significations have evidently been long lost. Such for example is hä^{e} as :—wă-kit-us, *I am asleep*, wă-kit-u-hä^{e}, *I am about to sleep;* "hä^{e}" here converts it into future time.

Again : wă-kă-rhä-rhu, *I am running;* wă-kă-rhä-rhu-hä^{e}, *I was running*, ran ; "hä^{e}" here converts it into past time.

These two tense signs "hä^{e}" are probably from different roots, one of which meant *will* and the other *did* or *was.*

In this search we have found possible roots with which to build up the neuter verb "be," even four not more radically different than *be*, *am*, *is*, *was*, of the English.

We believe that this is, if any, the only direction in which to seek out this hidden verb *to be*, and that if it exists, it is concealed in the obsolete verbal forms containing the root significations of said tense signs.

These forms might even in themselves prove to be the missing neuter verb.

But more profitable fields of inquiry, yielding more direct results, are awaiting the harvester and *to be* or *no to be* must still continue a question.

Accidents or Mode Signs of Verbs in the Iroquois Dialects.

By Mrs. Erminnie A. Smith of Jersey City, N. J.

[ABSTRACT.]

1. Movement in these dialects which in French is expressed by the auxiliary verbs *aller* and *venir*, and corresponds to the English participle in *ing* and the adverbs *toward* and *about*, is represented in

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Iroquois by final particles incorporated in the verbs making them verbs of movement. These might be termed mode and tense signs as the particles take the final inflections proper to the different time, as follows — Kninons, *to buy;* Kni-nonres, I *am about* to buy, etc.

2. Reduplication is the act of the verb repeated as repay, reconsider, etc. The mark of reduplication in several of the dialects is *S*, but in the Tuscarora Ç; as Kirh-hă, I *drink:* Ç Kirh-hă, I *drink again.* After the same manner can be explained affirmation, locality, causality, composition, frequentation, final relation, transition, habitude, resolution into contraries, partial reflexion, their change into substantives, etc., which furnish the material for the excessively long words to be found in all Indian dialects.

THE GREAT MOUNDS OF CAHOKIA. By WILLIAM MCADAMS of Alton, Ill.

[ABSTRACT.]

THE mounds referred to are in the locality known as the "American Bottom." The region so called is a strip of alluvial land in the state of Illinois, lying between the bluff and the Mississippi river, and extending from the city of Alton to a point below the city of East St. Louis. A map of the locality, showing the places and dimensions of the mounds, was exhibited before the section. The mounds are over two hundred in number, and embrace some of the largest in the United States. A group of seventy-two mounds on the Cahokia creek was specially considered. The central mound of the group is the largest: it is about a hundred feet high, and covers over twelve acres of ground. It is a truncated pyramid with terraces: its flat top has an area of one and a half acres. The surrounding mounds are thirty to forty feet high and most of them are square, in this respect differing from the conical mounds of Ohio. The mounds on the bluff seem to be of a different order, being only four or five feet high, and round or oval. Unquestionably the mounds of the Cahokia valley are artificial, being made of black alluvial earth, entirely different from the ground on which they rest.

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The author accounted for the fact that there were few mounds on the banks of the Mississippi river, by supposing that the mound-builders were afraid of their enemies beyond the stream.

Numbers of relics have been found in the Cahokia mounds, mostly of flint, some of them eighteen inches long. The finest is a white flint axe, which is of a smoothness and polish like ivory. In reply to an inquiry, the author stated that there had been considerable alluvial deposit formed since the mounds were built. The subsoil is a yellow clay loam: under the mounds is a floor of white sand.

ALTARS AND HIGH PLACES[1] AMONG THE EMBLEMATIC MOUNDS. By S. D. PEET of Clinton, Wis.

[ABSTRACT.]

THE significance of the term "High Places" will become apparent when it is once considered that the custom referred to in the Scriptures, was peculiar to those tribes in the east which were in a primitive condition, and whose religious cultus was at a low stage. They were either the worshippers of the sun and moon and heavenly bodies, or in the still lower condition of animal worship, and their customs may have been quite similar to those which are found to have prevailed among the so-called animal tribes of America. The term "animal tribes" expresses much, especially when we consider that tribes bearing animal names, and having their religion closely associated with animal totems, were common at the East in Syria and Arabia, as well as in Wisconsin and other parts of this continent. The religious customs of these tribes have not been so well known as of those which were more advanced; animal worship having preceded sun worship and being peculiar to prehistoric times. Whether the custom of erecting altars on "high places" can in the East be traced back from sun worship

[1] The title of this paper originally had no reference to the "High Places" which are described in the sacred Scriptures. The term was used to designate a certain class of mounds which had come under the observation of the writer. The use of it, however, seemed to give rise, at the time of the reading of the paper, to the idea that the Scripture custom was held in mind while the descriptions were given. This criticism is accepted, but the suggestiveness of the title is made the more apparent from it.

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to a more primitive cultus is a question. Here, however, they may be, and the subject becomes the more important from this fact.

There is one other point to which we call attention in connection with this hint at oriental customs. Human sacrifices were common in the East, and it has been found that these were also common among the primitive tribes of this country. It is an inquiry which we would make at the outset whether the altars and high places, of which we are to speak in this paper, were not places for cremation. Human sacrifices were common among the primitive races. Victims were seized upon and offered to cruel divinities. Such a custom was common among the tribes of Central America. There are cases where human bones have been found associated with altars in mounds. Yet we maintain that these altars and high places were used for the rites of cremation rather than of human sacrifice. There may be a danger of confounding human sacrifice with cremation, but, if we should find that ordinary burial mounds and the altars and high places were only designed for the different modes of disposing of the dead, we should consider that a satisfactory explanation of these altars had been reached. We bring out the facts contained in this paper with this distinction in mind desiring that our readers should consider that the sacrifices which were offered on the altars and high places were not true sacrifices, but cremations.

The custom of burying on high places was common with the native tribes in America as well as with the primitive races throughout the northern portion of Europe, in Great Britain, France, Germany and elsewhere. Nearly all the burial mounds throughout the Mississippi valley are in prominent localities on the summits of bluffs overlooking valleys, and in just such positions as are described as common for the cists and cairns of England and Europe. The explorations of certain gentlemen at Muscatine, Iowa, illustrate this point.[2]

Here, burial mounds are seen located on the bluffs on either side of the Mississippi river and extending for miles along the bluffs. The locality was favorable for residence, as the bluffs surround the limits of an ancient lake where there would be an abundance ot game and where the rich bottomlands furnished soil favorable for the cultivation of maize, and for a variety of products. This

[2] See American Antiquarian, Vol. III.

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custom of erecting burial places on bluffs and high places has given rise to the idea that there may have been a connection between the residences and the burial places. The Aryan custom was to have the family hearth and the burial place in close proximity, the Lares and the Penates being affiliated and their tokens associated. With these races also it was common to connect the sacrifice and worship offered to their divinities with their hearths; their tombs, temples and houses being closely associated. We throw out the suggestion whether some such custom did not exist also among the so-called Turanian races. Isaac Taylor maintains that these races were distinguished for their burial customs, though he does not define and describe, except in a very general way, what these customs were.

A few words on the connection between the altars in high places and ordinary tumuli may next be in place. There seems to have been a custom among the primitive races, of erecting tumuli or burial mounds on the hill tops, and the only difference between these burial places and the so-called altar mounds is in the fact, that, in the former, bodies were buried without the use of altars, while in the latter altars have been found, and with them various tokens of cremation. At times burial mounds contain tokens of burial at their base, and then at their summit other tokens which show that they were used for beacons or signal stations. This double use of the tumuli is very common. The writer has had his attention called to it in several places in Wisconsin. The custom oftener was associated with conical mounds, and yet effigy mounds have been discovered with evidence that burials were practised in connection with beacons and outlooks. The location of tumuli on high places probably led to the double use, as it would be convenient to light beacons on a mound already in existence. The same double use has also been noticed in connection with the altar mounds and their erection on high places may have been for the same purpose.

The custom of thus erecting the burial mound was common with the emblematic mound builders. There are many localities where burial mounds are so placed that extensive views are gained from them. Sometimes these views extend for many miles. There are mounds on Lapham's Peak, a prominent point in Washington county, from which a view can be had for twenty miles distant. The same is true of a line of mounds near Muscoda on the Wis-

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consin river. Burials on high places seem to have been common among the emblematic mounds. In reference to the sacrificial mounds this peculiarity of being erected on high places forms a point of resemblance to burial mounds, and so we might be led to consider the altars as designed for cremation rather than for sacrifice.

That cremation or sacrifice was practised mainly on high places I think may be shown. If there is need of proof of this point we might refer to the many sacrificial altars which have been exhumed in various parts of the country. Any one who will read the volume by Squier and Davis in the Smithsonian contributions[3] will be convinced that such High Places were common. The altars described by these authors seem mainly to have been associated with burial places, as many of the mounds which contain the altars contain also heaps, where evidently the ashes and remains of cremated bodies were deposited.

We will not say that sacrificing on *high places* was universal, for there are exceptions to it. Prof. F. W. Putnam has called the attention of the author to several cases which have come under his notice in Ohio, where altar-mounds were discovered on comparatively low ground. Yet we maintain that as a rule both burial-mounds and altar-mounds were placed on high ground.

They are generally placed on high bluffs or points of land, and are also frequently associated with village enclosures and with watercourses. The sacrificial altar at Paint creek, near Hopeton, had this peculiarity, that it was on the opposite side of the river from the village enclosure, giving rise to the thought that possibly there was the same custom of crossing the water to reach the places of cremation as existed among various races in the East.

Mounds have also been found by the writer in Wisconsin, which were evidently placed on bluffs overlooking village sites, which have proved to be altar-mounds, and it is a remarkable fact that nearly all of these have been on the opposite side of a river or lake from the village sites. This may have been a mere coincidence, but it has been found to be so frequently true, that it has given rise to the conclusion.

We are, however, to consider the distinction between altars and ordinary tumuli or burial-mounds.

Altars or sacrificial places were generally surrounded by enclos-

[3] Smithsonian Contribution, Vol. I, p. 54, plate XIX.

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ures. These enclosures were not always circumvallations, yet nevertheless served the purpose; but burial mounds were rarely surrounded by enclosures. The cases referred to above at Hopeton, Mound City, Paint Creek and elsewhere were marked by this peculiarity. Here, at Mound City, were a large number of mounds, twenty-four in number, surrounded by an enclosure. The authors Squier and Davis, in describing these, state that the open temples of the ancient Britons were embraced within parapets of earth, and that the sacred places of the Pacific Islands are surrounded, if not by earthen, by stone walls, or palisades. All the mounds in the enclosures were excavated by Squier and Davis and proved to contain altars. Another enclosure is described where a single mound was found, and this contained within itself an altar or place of sacrifice. Still another enclosure, with altar mounds, was visited by these authors at a place called Clark's Works, and contained seven mounds of great size, thirty feet high, one hundred and eighty broad at the base, and together forming a line five hundred feet long. A description of the altars is given in a separate chapter and is worthy of study.[4]

Among the emblematic mounds there are sacrificial altars which were erected within enclosures. These enclosures, like all others in this region, are formed by the effigies being grouped around an open space. The effigies do not form a connected wall, and yet they make an enclosure which is sufficiently guarded to give rise to the idea of the sacredness of the spot.

The enclosures containing altar-mounds are generally smaller in their area than village enclosures, and the altars are generally placed in the centre of the open space, making the distinction between the two classes of works manifest. Several such enclosures have been described by Dr. I. A. Lapham,[5] though their purpose was not apprehended by him.

A similar enclosure was also described by Squier and Davis. This was discovered by Mr. S. Taylor, and is called by him "The Citadel." "The figures comprising these are so arranged as to constitute a sort of enclosure of about half an acre area.[6] The ground is here prominent. From the top of the principal mound occupying the central group may be seen at least a hundred ele-

[4] See Smithsonian Contributions, Vol. I, p. 143.
[5] See Lapham's Antiquities, plate 40, plate 11, plate 10.
[6] See Smithsonian Contributions, Vol. 1.

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vations forming the boundaries of the Citadel." We give an illustration of this group (fig. 1).

FIG. 1.

Sacrificial place at Muscoda. S. Taylor.

Another group of mounds, which was designed probably for a sacrificial place, has been observed by the writer near Lake Koshkonong, and a figure of this is also given (fig. 2). This group, like the others referred to above, is composed of a series of effigies surrounding a central space within which are two mounds, one a beacon-mound, No. 4, and the other an altar-mound, No. 3. These effigies surround the altar forming a sort of guard to it. These effigies have the shape of familiar animals; a turtle, No. 1,

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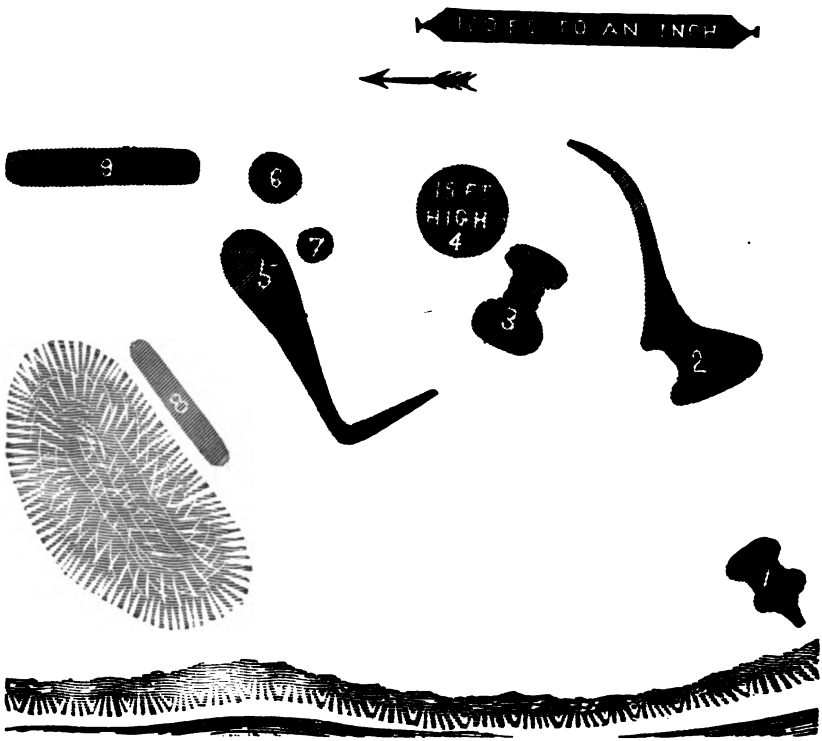
a panther, No. 2, and a cat-fish, No. 5. The dark spot represents a sink hole, and the shading the bluff and water-line. The ground rises gradually, so that the central mounds are higher than the effigies, yet it can be seen that the effigies were designed as

FIG. 2.



Altar-mound at Lake Koshkonong.

guards to the altar. The view from the beacon is extensive, and it is not difficult to imagine that the sacrifices practised upon the altar were seen from a great distance. The group is on the banks of the lake, and commands a view of the opposite side where there are many other mounds surmounting the hilltops. The locality is attended with many other effigies and earthworks, showing that the spot was a place of great resort. None of these mounds have been excavated, but the enclosures resemble others

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which have been found to contain altars, and we therefore refer to them as probable specimens of sacrificial places.

We will now call attention to the facts which we have discovered to prove that there were altars on high places among the emblematic mounds. The first locality to which we refer is one which has been explored, but the peculiarity of which as a sacrificial place we claim to have discovered. It is situated on a high ridge which forms a dividing line between Lake Monona and Lake Wingra, near Madison, Wisconsin (fig. 3). This locality is in plain sight of the railroad depot, of the University, of the State Capitol, and of the hotels in the city. The ridge is a contorted, grotesque, abrupt line of high gravel knolls, every one of which has mounds on its summit, the shape of the mounds being strangely conformed to the shape of the knolls. This species of animism, by which the mound and the earth should be made expressive of the same thing is peculiar. Here is the "double" which is common to all animistic faiths. We shall not dwell on this point, however, as another paper will illustrate it, but refer to it here as it was one item which led to the discovery of the sacrificial place. The mounds on these knolls were, many of them, burial-mounds, for skeletons have been exhumed from them. They were also, evidently "Lookout mounds," for each mound has its own field for observation. The views which are presented from the different mounds are as unlike as can be, the broken character of the surrounding country requiring particular points of observations to secure particular views. Mounds not over ten or twenty feet apart will give entirely different views.

In the midst of this ridge at its highest point was discovered a group of effigies which were remarkable alike for their shape, their relative position, and their conformity to the ground on which they were placed. It will be noticed that several of the mounds have the shape of animals, one of them a serpent, No. 16, another a panther, No. 17, another a fish, No. 18, and another a nondescript creature, No. 10. These are all conformed to the shape of the ground. The serpent forms the very spine to the contorted ridge, and follows the outline and rise of the ridge, so that it seems to be crawling upward toward the altar. The panther is situated on a spur of the ridge which runs out to the east of the central group. The effigy forms the summit of this spur and follows its outline. The animal lies upon its side, its

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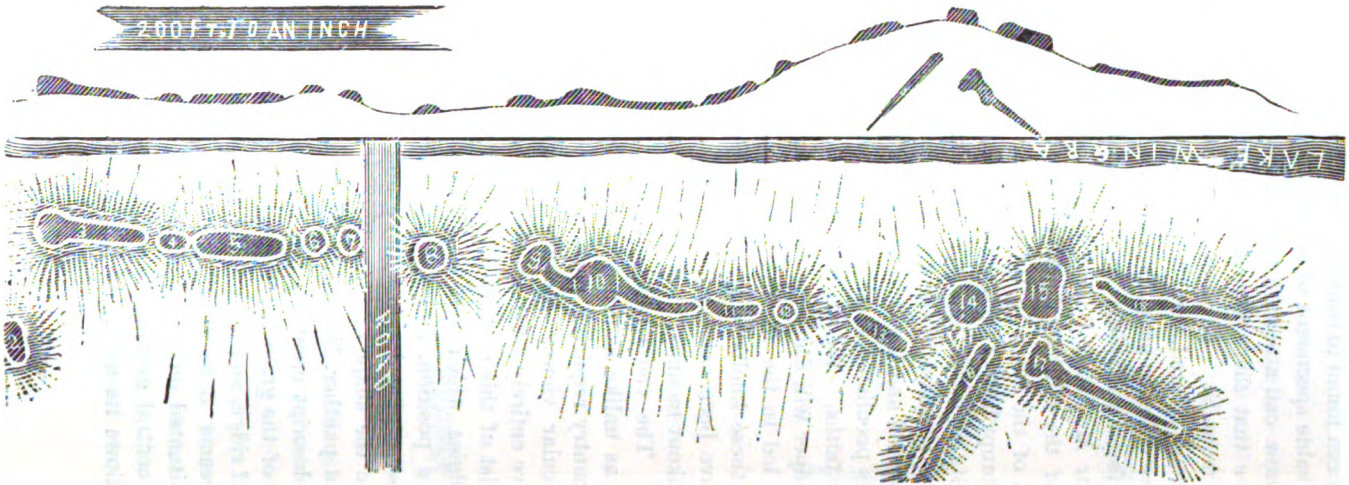
FIG. 3.



Altar-mound on Lake Wingra, near Madison, Wis.

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tail reaching to the very foot of the hill, its huge body lifted high up on the summit on the ridge, and its head situated at but a short distance from the altar. The long, tapering mound follows the outline of the ridge in another direction, the three effigies forming a sort of guard or enclosure for the central group. The relative position of the mounds indicates that there was a design in the mind of the builders to make them serve three uses, namely: to guard the altar, to show the shape of the ground, and to give the shape of the totem effigies.

In the centre of the group is a signal-mound or beacon. This is situated on the highest point of land and commands a view of three of the beautiful lakes which surround the locality: Lake Monona to the east, Lake Wingra to the west, and Lake Mendota to the north. Another lake is situated but a mile or two to the south, but cannot be seen as high land intervenes. The point, however, is central, and is surrounded by an amphitheatre of hills and seems to have been the centre of a large number of earthworks and effigy mounds. There are emblematic mounds on all the surrounding hills. The Capitol occupies the site of mounds formerly existing on that hill. The University is surrounded by effigies which were formerly more distinct on that hill. The Asylum, across Lake Mendota, four miles distant, is now surrounded by a large number of striking effigies. The stone quarry between this ridge and the university is surrounded by a large number of effigies. Greenbush, within gunshot, has another group of effigy mounds. The Home of the Sisters of Charity, across Lake Wingra to the west, has another series of effigies. The dairy farm and fair grounds, between the Charity Asylum and the University, have another group. Back of these, on the summit of the high bluffs to the west, is the cemetery, where there is another group. To the east, there are effigies on the hills between this ridge and the assembly grounds at Lakeside. On the opposite bank of Lake Monona, three or four miles distant, is the shooting park; here is another group. Still farther east in Mill's Woods are many other striking effigies. At the extreme east end of Lake Monona is a series of gravel knolls, on which there are also effigy mounds, and in the cornfields back of these, many burial mounds. The ridge which bounds the horizon to the east has another series of mounds. To the southeast of these, near the outlet of Lake Monona, is a very interesting group of effigy mounds

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and earthworks; and to the northeast, near the inlet of Lake Monona, is still another.

The locality seems to have been well chosen, as its central position makes it conspicuous, and its isolation and the peculiar shape of the ridge would make it a prominent object in the landscape. The sheets of water come so near to the foot of the ridge as to give an unimpaired view, and the height and shape of the knolls make it even now an impressive spot, just such a spot as would be regarded with peculiar awe by a superstitious primitive people. It needed only the kindling of fires on the summit to throw a glare of light across the water and to fill the whole landscape with shadows. We can imagine that it would be a very weird and wild place for the erection of a burial mound; a remarkably favorable place for the lighting of beacon fires and especially favorable for the practice of sacrificial rites. Of the group itself we do not need to give a very extended description; figure 3 will illustrate this. The beacon mound is the central object. Near this is what we call the altar; it has a peculiar form resembling that of a tortoise shell, but without any of the protuberances which would represent the limbs and other members of the tortoise. This so-called altar has been exhumed and remains taken from it. Certain members of the Wisconsin Academy of Sciences have explored this mound, and we quote from their report.[7]

"Mound No. 1 is pear-shaped, being 78 feet long and 55 feet wide. It was composed of alternate layers of mold and clay to the depth of 6 feet, then a layer 1 foot thick, succeeded by alternate layers of clay and mold to a depth of 9½ feet where the natural surface is reached."

"The penetration was carried three feet below the natural surface through layers of bowlders and coarse gravel. *A fire place two by two and one-half feet*, with a layer of charcoal and ashes 4 inches in thickness, was found at a depth of 5 feet. In this was a piece of cloth partially burnt which for the most part crumbled to powder on exposure to the air. Both on the north and south sides fragments of bones occurred of from 3 to 5 feet from surface in an advanced state of decomposition. In the centre at 2 feet above natural surface were obtained, nearly broken down by de-

[7] See Transactions of Wis. Academy of Sciences, Vol. III, p. 10, for 1875-76.

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composition, the femurs, tibiæ and fibulæ of a single skeleton, corresponding to 6½ feet in the height in the living subject."

"Mound No. 2 (the beacon mound) is a round mound 40 feet in diameter and sloping to the base of No. 1. The line joining the centres is 64 feet long. Two fire-places were found: one at 3 feet and the other at 5 feet below the surface. Dimensions the same in each, 3 × 6. In the lower one were partially burnt bones and the ashes. At 6 feet were found some pieces of pottery and a mass of bones consisting of fragments of four or more skeletons in a tolerably good state of preservation, corresponding in height to 5 feet 8 inches to 6 feet 6 inches in the living subject. Photographs of the two crania were secured and figures of these are given with the report."

This is the description on which we base in part our supposition that the locality was a sacrificial place. The figures illustrating these objects may be seen in the report, and so we omit them here.

One other locality where altar-mounds have been discovered may be worthy of mention here. It is a mound situated on the north side of Lake Mendota. This is also one of a group which was located on high ground and attended with a lookout mound. The first description of it was given by Dr. J. N. De Hart.[8]

The largest pyramidal tumulus of this group (fig. 4) measures one hundred and eighty-eight feet in circumference, and thirty-five feet from the base to its summit. From its summit we have an extended view of the surrounding country several miles in every direction. The altar-mound, to which we refer, is located a few yards from this beacon mound. This mound is about five feet in height and one hundred and twenty-five feet in circumference. We give a representation of this mound and quote from Dr. De Hart's report. "After removing the surface a layer of gravel was found one foot in depth, and on the summit a layer of sand dipping downwards into the layer beneath it. Following this was a deposit of black loam three feet deep, and underneath this was another layer of gravel, and then a deposit of earth. A thin stratum of gravel was then removed, when ashes, charcoal and flints were found lying upon an altar of stones. The latter rested upon a bed of yellow clay. The altar was about one and a half feet high, three and a half feet in length, and two feet in width.

[8] See Transactions of Wisconsin Academy of Sciences, Vol. IV, 1876, p. 188. See also American Antiquarian, Vol. I, No. 4, p. 190.

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The excavation was continued downward to the depth of three and

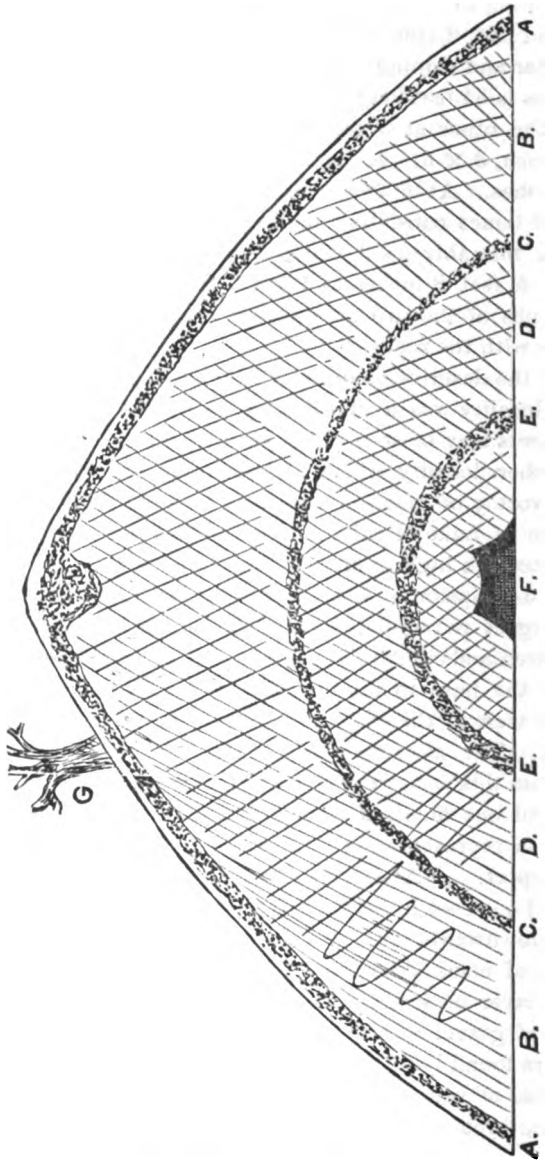

FIG. 4.

Altar-mound on northeast shore of Lake Mendota, near Madison, Wis.

a half feet below the surrounding surface, but nothing further was found."

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AN ANCIENT VILLAGE OF THE EMBLEMATIC MOUND-BUILDERS; CACHES GUARDED BY EFFIGIES; EFFIGIES GUARDING THE VILLAGE, AND SACRIFICIAL PLACES NOT FAR AWAY. By STEPHEN D. PEET of Clinton, Wis.

TYPICAL SHAPES AMONG THE EMBLEMATIC MOUNDS; THE DIFFERENT ATTITUDES EXHIBITED BY THE SAME ANIMAL. By STEPHEN D. PEET of Clinton, Wis.

GAME DRIVES AMONG THE EMBLEMATIC MOUNDS; DEER DRIVES, APPARENTLY CONNECTED; BUFFALO DRIVES WITH EXTENSIVE OUTLOOKS; HIDING-PLACES FOR HUNTERS, AND EMBLEMS ACCOMPANYING THE DIFFERENT DRIVES. By STEPHEN D. PEET of Clinton, Wis.

THE CORRESPONDENCE BETWEEN THE PREHISTORIC MAP OF NORTH AMERICA AND THE SYSTEM OF SOCIAL DEVELOPMENT; THE GRADES OF PRIMITIVE SOCIETY, AND THE CONDITION OF THE NATIVE TRIBES COMPARED. By STEPHEN D. PEET of Clinton, Wis.

PERSONAL OBSERVATIONS OF THE MISSOURI RIVER MOUNDS FROM OMAHA TO ST. LOUIS: CONSIDERED FROM A GEOLOGICAL STANDPOINT. By E. P. WEST of Kansas City, Mo.

[ABSTRACT.]

THE mounds extend from Omaha, Nebraska, to St. Louis, Missouri, in an unbroken chain and are invariably associated with the Loess and Terrace formations on the Missouri River, and are probably coëxtensive with them or their equivalents in other localities. But this fact has but little significance without other facts which stand associated with it. The black vegetable mould is as thick on the mounds as on the adjacent land. The trees grow-

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ing on them are as large and as old as those of the surrounding forests. But all this might be covered by a period of 500 years. Other facts which may be superadded are more conclusive. Stone implements have been frequently found in the Loess from three to thirty feet deep under conditions that leave but little doubt of their having been lost in the old Champlain lakes and covered in where found by the super-accumulation of their deposits. Human bones were found at Kansas City in a Loess hill, near its summit, eighteen feet below the surface and within ten feet above the base of the deposits under conditions to lead to the belief that they were engulfed and buried in the same way. A vase of antique pottery was found in a Loess hill near its summit, fifteen feet below the surface, in clay which had every indication of having remained undisturbed from its primitive deposition, at White Cloud, Kansas. Another vase precisely similar in every respect was found in an artificial mound on the summit of the bluff overlooking that place and the Missouri River about half a mile distant, but at a greater elevation. A vase of the same antique work was found in sinking a well on a broad low terrace at Manhattan, Kan., at a depth of nine feet. Dr. Parr of Weston, Mo., found a similar vase in opening a mound on the summit of the bluff overlooking that place and the Missouri River, which contained the vertebræ of a fish and some shell beads. The modern Indian erects his wigwam on the recent alluvium, and in the valleys and upon the plains wrought out by all the geological ages, with equal indifference, showing no preference whatever for the Loess bluff or the terrace tables. Not so with the Missouri River mound-builders. They chose these formations to the exclusion of all others. The preference could hardly have been from any peculiarity of the soil or of the formations themselves, and we must look to extrinsic conditions to explain their choice.

The Loess was deposited in lakes of still water and the terraces were formed by shore washings, and exposed by subsequent elevations of the land or subsidence of the water. May we not, then, look to these lakes for an explanation of the preference? May we not regard the Missouri River mound-builders as a race of lake-shore dwellers, made so from considerations of food supply? It is probable that all of the mounds in Kansas, Nebraska, Iowa, Missouri, Illinois, Wisconsin, Michigan, Indiana and Ohio north of the 40th degree of north latitude are of about the same age and correspond

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within a few hundred years, at most, with the age of the Missouri River mounds, and stand associated with equivalent geological formations, *i. e.*, that they were all erected fringing on lakes which had a synchronous existence in the Champlain epoch.

The occupancy of the mound-builders began before the subsidence of the Missouri River and Kansas lakes, otherwise their remains and implements would not be found, as they are found, in the undisturbed primitive deposits. And the changed conditions which followed the subsidence preclude the idea of their long continuance afterwards without changed habits and modes of life. Outward physical conditions in a great measure determine the modes of life of a primitive people, and this simple race, turning northward after the close of the ice reign, found the warm Champlain lakes filled with fish, inviting an occupancy of their hospitable shores. Here they erected their mysterious abodes and drew their principal food supply from the lakes. But time moved on with its inevitable changes. The lakes were drained and the supply of food they afforded was diminished, while the productions of the land were increased in a corresponding ratio. Conditions were totally altered and the mound-builders suffered extinction or else were forced to change their modes of life. At any rate, their distinctive character was lost long before the European touched foot upon this continent.

Were they exterminated by nomadic neighbors, or did they become themselves nomadic in their habits? We may never know, but their total and tragic extinction is most probable.

THE MOUND BUILDERS IDENTIFIED. By JOHN CAMPBELL of Montreal, Canada.

[ABSTRACT.]

IN this paper it was taken for granted that the ruling tribes of Mexico and the Mound-builders belonged to the same race. This parent race is alluded to in the Mexican annals as that of the Citin. The author derived the American Citin, whose works first appear on the borders of Alaska and British Columbia, from the Khitan of the Chinese and Corean historians, who, together with

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the Tchoutchis, once occupied a great part of Siberia, took possession of northern China, and sent colonies into Corea. Tracing the Khitan backwards he found an earlier seat of their empire in the great mound country at the head-waters of the Yenesei in Siberia, the mounds of which bore among the Tartars the traditional name Li Katei, or the sepulchres of the Cathayans. Through Tartary and Western Thibet the Punjaub was reached and a more ancient habitat of this race indicated in the country possessed in the time of Alexander the Great by the Cathaei, whose capital was Sangala. Thence along the line of the Hindoo Koosh mountains and the southern shores of the Caspian, he arrived at the original home of the race in the Caucasus, Armenia, Syria and Mesopotamia, where it was known to the Assyrians and Egyptians as that of the Khita or Kheti. The Khitan empire existed on the borders of Palestine and Syria in the fifteenth century B. C., and was destroyed by the Assyrian Sargon in the year 717 B. C. Soon afterwards it was reëstablished in northern India where Alexander found it still flourishing in 327 B. C. Aryan encroachments did not succeed in wholly overturning this empire till the fourth century A. D. Then it revived in Chorasmia and after a short existence removed its seat on account of Tartar invasions to Siberia. Another eastward movement, the result of foreign pressure, marked the eighth and ninth centuries, during which period Corea and Japan must have been colonized and America discovered. In the eighth century the Toltec empire in Mexico began, and in the eleventh the Aztecs left their Asiatic home.

The writer called attention to the agreement of the various historical peoples identified as members of the Khitan family in physical features, language, religion, government and arts, as mound-builders and the possessors of a peculiar form of writing. He then proceeded to compare the Khita hieroglyphics of Hamath and Carchemish in Syria, with those of the Siberian inscriptions from the Yenesei, the Mound-builder characters on the Davenport, Brush-Creek and Grave-Creek stones, and the well-known Aztec hieroglyphics, exhibiting their identity as syllabic types, and confirming it by comparison with the Cypriote and Corean alphabets derived from the same source. In this connection he referred to translations of several of these inscriptions, Hittite, Siberian and Mound-builder, published by him, the key to their transliteration being the Aztec hieroglyphic system and to their translation the

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Japanese language, which he regarded as the standard of Khitan speech.

He then presented a classification of existing peoples in the Old World and in the New whose primitive stock as evinced by language, religion and tradition was Khitan, however modified by hybridism in physical feature. The following is the classification :—

In Europe.	Basques and Picts.
In Asia.	Caucasians, including Georgians, Circassians, Leshgians, Mizjeji, and probably Ossetes.
	Turanian Indians, including Pulindas, Quasi-Pulindas, Kariens.
	Siberians, including Yeneseians, Yukahirians, Koriaks, Tchuktchis and Kamtchatdales.
	Japanese, including Loo-Choos, Ainos, Coreans and Japanese proper.
In North America.	Dacotahs.
	Huron-Cherokees.
	Choctaw-Muskogees, including Natchez, etc.
	Pawnees
	Paduca-Pueblos.
	Yumas.
	Aztec-Sonoras.
In South America.	Lencas of Honduras.
	Chibchas of New Granada.
	Peruvians.
	Chilenos.

The writer referred to the former existence of a hieroglyphic syllabary among the Creeks and suggested that the Cherokee alphabet of Sequoiah had been derived from some such source. He maintained that the Sanscrit of American speech, by which languages of apparently diverse origin might be unified as to source, must be looked for not in America, but in the old world.

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METRICAL STANDARD OF THE MOUND-BUILDERS, DEDUCED BY THE METHOD OF EVEN DIVISORS. By CHARLES WHITTLESEY of Cleveland, Ohio.

[ABSTRACT.[1]]

AFTER giving an account of the principal surveys of the ancient works of the state of Ohio, the author stated that these large groups of earth walls mark the sites of ancient cities. In their original state their greatest height was probably from fifteen to eighteen feet, generally without ditches. Some of them are now fourteen feet above the surface of the ground, extending at various heights more than a mile in length. Those at Marietta, North Bend, Portsmouth, Newark, and near Chillicothe, are yet so well preserved that they can easily be traced. They show long lines of embankments, rectangles, squares and circles, with entrances or gateways; and also mounds, with a square, rectangular or circular base, and frequently truncated summits. There are at least fifty combinations of this kind constituting pueblos or ancient towns within the limits of Ohio, and many hundred single mounds, forts and isolated works, the objects of which are still conjectural. There are rarely effigies in earth, and a few enclosures and mounds of loose stones. The engineers who laid out so many mathematical figures must have had a common unit of measure.

The average of the outstretched arms, from finger tip to finger tip, has long been recognized to be a fathom of *six English feet* generally used in nautical matters. It can be traced as far back as the Romans. There is a name for it among the ancient Teutons and Scandinavians. The origin of the English yard is not known, but the probabilities are that it is based upon the fathom divided in half.

If we look to the human arm as the datum of the mound-builder standard, and assume for the purpose of discussion that a rod or stick of two and one-half or of *five English feet*, represents very closely the measure used by that people in laying out their earth works, we have sixty English inches.

The best determination of the average human arm gives about thirty inches from the shoulder joint to the tips of the second (or long) finger, which is half of sixty inches.

[1] Printed in full in the Tracts of Western Reserve Historical Society, Cleveland, 1883.

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If the outstretched arms truly represent the fathom of two yards, the average breadth of the chest, between shoulder joints is *one foot*, or twelve inches. Laying the palms flat, with the ends of the thumbs touching each other, the spread of the hands is a close approximation to a foot; one palm and its thumb is a *half foot*, or six inches.

Is this coincidence accidental? That any mound-builder unit should correspond to the English foot, or inch, must be accidental, unless there is something in common in the derivation of the standards. Some of the banks, platforms and forts, where the forests still remain over them, appear to be nearly perfect; but, of course, the upper crests have been shortened by exposure, and the base lines increased. The surveyor generally finds them in such a condition that fractions of feet are not reckoned, except in regard to height. Of course the superior lines are all short, more or less, and the lower ones are all too long, as compared with the originals. Where the measurements are taken along the middle line of the present summits, from angle to angle, the distances must be nearly the same, as the originals, which were no doubt measured on the natural surface, along the middle of their bases.

A survey along the middle of the surface of the present banks would be in a vertical plane directly over what I presume to be the guide line of the mound-builder engineers. A perimeter determined in that way cannot materially differ in length from the original, but a very large proportion of the works are not regular figures, with equal homologous sides. For the purpose in view, only such as are very near perfect squares, circles or rectangles will be used.

Recently Professor O. H. MITCHEL, of Marietta College, at the instance of President ANDREWS, made a survey of two prominent earth platforms, or truncated pyramids, in the group of works at Marietta, O. Professor MITCHEL reports that their flat surfaces are in good preservation — one 146½ by 138.20 feet, the other 118.14 by 200. The slopes are not of uniform grade. There is no difficulty in making exact measurements, which were determined by two sections of his engineering class, but they differed half a foot in the position of one of their corner stakes.

Selecting from all the authorities within my reach such lines as appeared to have been measured with care, I found them to be *eighty-six* in number. *Sixty-seven* of them are the sides of seven-

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teen platforms, squares or rectangles, varying in length from 45 to 1,150 feet. *Eleven* are the radii of circles 88 to 860 feet in diameter. Portions of regular works that are so much injured that good measurements of all the parts cannot be made, having 50 to 950 feet in length, furnished the remaining *eight* lines. Long lines are more reliable than short ones, because there is always some doubt as to the points where to begin and to end; and also as to the angle where the middle line is used.

In Ross county there are three square embankments of the same size having 1,080 feet on all sides. Below is the result of the division of 87 lines, showing the results in a tabular form:

Percentage of lines divisible by 2½, and its multiples.

2½ feet	fifty-three per cent.
5 "	fifty-five "
10 "	fifty-seven "
15 "	forty-five "

Divisible by 3 and its multiples.

3 "	thirty-six per cent.
6 "	twenty-three "
9 "	twenty-three "
12 "	four "
Divisible by 4	twenty-five "
" " 8	fifteen "

It would seem to be a certainty that the ground plans should have been laid out by a rod, which was not used in fractional parts. If a large number of base lines are found to be divisible by one figure, it would be a very strong inference that the measuring rule of the builders was about that length. But, because we use feet and inches to make our measurements, that affords no proof that the ancients made use of feet and inches. We have no other mode of making comparisons, but there were many other units which they could have used.

The length of the average human arm, and of the pace, are very near to *thirty inches*, or *two and one-half feet*. In some countries the pace is reckoned from the track of one foot to the next one of the same foot, or *five feet*, equal to sixty inches.

Because the rule or measuring stick of the mound-builders appears to have been equal to a given number of our inches, and derived from the arm or pace; it by no means follows that its subdivisions were equal to our own, or that they knew of either of

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them. Admitting that their metrical standard was thirty inches, it may have been divided in *half*, equal to fifteen inches; or into *quarters* of $7\frac{1}{2}$, and *eighths* of $3\frac{3}{4}$ inches. These spaces are divisible by *half* an inch, by *three-fourths* of an inch, and by an *inch and a half* as readily as by an inch; except the eighth part. What the subdivisions really were, we are left wholly to conjecture.

Methods of Arrow Release. By E. S. Morse of Salem, Mass.

[ABSTRACT.

The author recited the rules at present applied in the American system of archery; the bow being drawn with the right arm, the arrow being placed on the left side of the bow, and three fingers being used to hold the arrow. Among the Japanese, a different system prevails: the arrow is placed on the right side of the bow as it is held perpendicularly, and the drawing of the bow is performed with only the thumb and one finger on the shaft. China, Japan, and the Corea are alike in this manner of drawing the bow. Among Indian tribes, the methods of arrow-release differ very widely. In general it may be stated that our system of arrow-release (which the author designated as the Saxon method) is substantially the same as that of the majority of European races, the modifications of the system among them not being important.

The Japanese use a glove with a heavy thumb, and sometimes a heavy ring on the thumb. Mr. Morse exhibited the Japanese archery-glove. It has a filling of wood and pitch in the thumb, which aids in grasping the arrow. He considered this glove the best of its kind.

Our system of three-finger release is certainly as good as any other, and probably is the best. With this system our archers — for instance, some in Ohio — are able to outshoot any Indian, tried by all the usual tests. As to the methods of stringing the bow, the author had not been able to find much uniformity. A number of different modes were exhibited.

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The Kitchens of the East. By E. S. Morse of Salem, Mass.

[ABSTRACT.]

The author, during his travels in eastern Asia, had made some observations on the cooking-apparatus there in common use. The Japanese largely employ a mere fireplace, over which the vessels containing food are suspended by hooks: they have, however, two or three kinds of regular stoves of different designs. In China, stoves of a definite character are in use: one was found in Canton which was very elaborate; it was long, and had numerous openings. In Singapore, there appears to be only one kind of stove, and it is of decidedly primitive construction. In fact, it is little more than a rough trough filled with earth and sand, on which are laid rough stones selected with reference to pots of various sizes; and the fire is built among the stones. The kitchens in which these constructions are found are invariably very dark and dirty. In northern Java the author found a stove made of arched clay, as half an earthen pipe would be if cut through the axis of the cylinder. This half-cylinder is set with the open part down, and fire is built under its arch. Holes are cut through the crown of the arch, to hold some of the pots, while others are merely set upon the surface.

In-door Games of the Japanese. By E. S. Morse of Salem, Mass.

[ABSTRACT.]

Among the simple in-door games of the Japanese are some that are played with balls, jackstones, and cat's-cradle; but all these are more elaborate than with us, and the cat's-cradle goes through a far greater variety of changes. The author believes that the greater intricacy of Japanese simple games is due to the fact that older people take more interest in them. Among these games, there is one similar to "Simon says thumbs up;" there are tricks with the hands, much like our own; and there are numerous games of striking hands, which appear easy, but require much practice to acquire adroitness. There are many games that test strength or

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endurance: among them are some in which ears and noses are pulled, and others where the competitors each hop on one foot, and try to push their rivals over.

They have a very elaborate game entitled Yo. The pieces are placed on intersections instead of on squares. It frequently takes a month to play one game, and the players often deliberate over a move for an hour or two. Experts in the game acquire a wide reputation. Japanese chess is probably the most intricate game in the world. The board has eighty-one squares, and twenty pieces are used, which have moves somewhat like our own, though none are exactly similar. The pieces change in grade when they arrive at a certain position on the board. The strangest feature of the game is, that either player can take any piece which has been captured from him, replace it on the board, and use it against his adversary. This makes the game utterly bewildering to a foreigner.

The Japanese have no games with spotted and court cards like ours; but they have a card game of "authors," which compels players to cap verses of classic poetry.

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SECTION I.

ECONOMIC SCIENCE AND STATISTICS.

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ADDRESS

BY

DR. FRANKLIN B. HOUGH,

VICE-PRESIDENT, SECTION I.

THE METHODS OF STATISTICS.

I INVITE your attention to a few thoughts upon the *methods of statistics* — using the term "statistics" in its broadest sense, as a "statement of facts."

The subject naturally divides itself into two distinct operations: the collection of the data from which information is to be obtained, and their classification in a manner that shall without error, and with the least labor, present the results in a form most convenient for use.

Commencing with the first of these, — the collection of facts, — it would be needless to remark, that everything depends upon the simplicity, accuracy, and completeness with which they are obtained, and that by no subsequent operation can their errors be eliminated, or their deficiencies satisfactorily supplied.

It may be remarked, in general, that no intelligent person, business firm, or corporation, can safely begin any enterprise, — nor can any government, from the lowest municipal to the highest national form, undertake any measure with prudence, — without first knowing all that can be ascertained beforehand concerning it.

In private business, inquiries are naturally made as to the cost and the profits. If it requires the use of a raw material, the parties will endeavor to make themselves sure as to its abundance, — the probability that the supply will be maintained, — or, if it be of limited amount, the quantity, and the time that it will hold out. They will need to know the changes that may happen in amount, quality, and cost; and similar inquiries will be made as to the ex-

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penses that may be incurred while in their hands, — the chances of loss, or of change in value, — and, finally, the extent of the demand for whatever may be the product of their skill, industry, and investment, its probable permanence, and its tendencies to change.

These questions, being well considered in the beginning, will enable the careful operator to avoid losses from imprudent investment, from over-supply of the markets, or from the depression of receipts below the limits of cost.

By a train of reasoning analogous to this, those intrusted with the government of towns, cities, or states, may determine as to how far the cost and maintenance of public enterprises will be justified by the results; but with this difference, that the benefits or profits, instead of being measured by a money value, are often to be found in an advancement of the public welfare, and in the security, convenience, and prosperity that may ensue.

But whether in private enterprise or public undertaking, we may attribute success alike, in both, to an attentive notice of the facts and circumstances upon which they depend; and, if loss or failure follow, the reasons may very generally be traced to ignorance or inattention as to the facts and probabilities that should have been known beforehand.

These thoughts lead us directly to the point we are first to consider, viz.: How shall the knowledge of the required information be obtained? In the primitive way (and for a small business this may be the best one), the person will, from his own observation, "look over the ground," and consider the various points to be taken into the account. He will make inquiries of others, as to the supply, demand, prospects of competition, and the like; and thus accumulate a certain amount of information, upon the extent and accuracy of which, his success or failure will in a great degree depend.

Advancing a step farther, we find, in most great industries and interests of the country, that those in the same business or pursuit, whether in the arts or sciences, or in financial operations, however they may be influenced by local rivalries or petty jealousies, are constantly tending to the formation of associations or societies, for the advancement of their common interests. They meet for the discussion of methods by which expenses may be saved, or profits increased. They inquire of one another as to their experience or observation upon doubtful points. They seek to gather light and

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aid from science to stimulate and reward invention, and to excite rivalries in the comparison of improved products. They discuss financial and national questions that may effect their welfare; and not unfrequently they appoint committees or agents, from their own number, to gather statistical facts and details for their own use and guidance.

We consider the information thus obtained, as deserving high rank in point of accuracy. It is chiefly taken from records, without a motive for concealment or evasion, and with a full knowledge that self-deception and loss would result from error, whether above or below the truth.

From this combined experience, each member who participates obtains a standard for comparing his own results with the general average. He cannot afford to fall below it, and he has the strongest motives for reaching the highest limits that have been reached by others.

Still these statistics, however accurate they may be, are necessarily special, and often technical in their nature. They cannot be compared with those of another business, and may be incomplete within themselves, as naturally relating to methods, rather than to financial details. They might show *how*, rather than *how much*. They will seldom contain a balance-sheet of profit and loss, or anything that would advance the fortunes of a rival in business, or reveal the secrets of an unprofitable enterprise. We must receive them as we find them, — good only as far as they go.

Besides these associated business inquiries, prompted and guaranteed by self-interest, we find various others, voluntarily undertaken with reference to particular subjects, often for the promotion of a moral, religious, benevolent, educational, or political object and ranging in value all the way between the accuracy of statements taken from records, or gathered by faithful inquiry, by chosen special and zealous agents on the one hand, and the random conjectures carelessly returned by those who know but little, and care still less, about the subject of inquiry, on the other. It would be wholly impossible to assign a scale of value to statistics thus obtained, where everything depends upon the circumstances of the case, and the accuracy of information on the part of those who make returns,— the fulness with which they are reported and the care with which they are combined.

We have another class of non-official statistics collected and pub-

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lished by private enterprise, for the information of particular trades or professions, or for use by the general public; their reputation and success depending wholly upon their accuracy, and being brought to the test of local and personal knowledge every day and everywhere, we may naturally expect them to be as accurate as they can be made. In this class, we may include directories, trade and market reports, financial transactions, and the current commercial statistics generally.

There may be instances where they are tainted with a suspicion of private or speculative motive: but such is the vigilance of rival enterprise, that detection will quickly follow, and an exposure would at once degrade a reputation for independence and impartial statement, to the rank of a private job for a speculative end.

Exhibits openly made, for the avowed purpose of presenting the favorable side of a business enterprise, may be taken for what they are worth, and are often trustworthy; but, when concealed under a false pretence, they deserve suspicion, and, when exposed, they generally injure the interest that they represent.

The best of these statistics are taken from records, and are entirely correct; others are collected by special agents, and should be approximately near the truth; and there is still another class, made up from the estimates of those supposed to know the facts, and which must wander more or less from the actual conditions that they attempt to represent.

It may be said of all of them, that their greatest value is for present use. They quickly pass by, to give place to the next issue, and remain only as historical records; but, as such, they still afford a most valuable means of comparison between the present and the past, and become landmarks of progress, ever instructive to those who may be seeking to trace the origin and growth of our industries and our resources; and now and then they are recalled as precedents, where new questions arise, under circumstances deemed similar to the past.

We will next consider some points relating to inquiries undertaken by authority of government, either for the intelligent discharge of its own functions, or for general information, the good of its citizens, and the advancement of knowledge among mankind.

We may, in general, remark, that nothing can be properly done, in the machinery of government, without leaving its record. If money or property is received, there is an entry; if a payment is

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made, or if property issued, there is also an entry, and a receipt to prove it. In short, the whole theory of our government involves the necessity of a record of every official transaction; and it is only in the cases of intentional fraud, or gross neglect, or unavoidable accident, that the history of every public act cannot be traced from these records.

A record, to be trustworthy, should be made at the time of transaction, and while all the facts as to time, subject, and amount, or other points of statement, are fresh in mind. Nothing should be trusted to the memory, and for record at more convenient season. It should be concise, and easily understood, and may often be very greatly assisted by tabular arrangement.

The summaries of these records, as published by the government, are, we believe, with few exceptions, entitled to great confidence, as far as they present transactions done by authority, or passing under the notice of government agents.

We may classify the official statistics of the government under the following heads:

First, Summaries of current business, published annually or at shorter intervals.

Second, Periodical inquiries made at wider intervals, as in the census, and requiring special agencies for their execution.

Third, The inquiries made by experts, or by special commissions or agencies created for a particular purpose. This class is sometimes associated with one or the other of the preceding.

Taking from among these classes the census, as one of the most important, let us notice some of the methods by which it has been taken.

The earliest returns that I have found, in colonial times, were made by sheriffs and constables. At a later period, the national census was for a long period taken by the marshals of the district courts, or their deputies, — officers whose duties are quite analogous to the former; and this practice of assigning the task to sheriffs still prevails in several of the states.

In many other cases, assessors discharge the duty. In New York, before 1855, special agents were appointed by local authorities; and, commencing with that year, they have since been appointed by the secretary of the state. The appointing power has been vested in state boards, in boards of county commissioners, and in the judges of inferior county courts. Assuming what I

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think all will admit, that census inquiries should be made entirely free from any suspicion that some tax or some personal liability is to be incurred, it is evident that an assessor cannot question an ignorant man about his property and his crops, without exciting his fears that some tax is to be laid. The sheriff or the constable seldom makes a professional call, except to serve some papers or make an arrest. There is, therefore, a strong reason for appointing persons who are to make the census inquiries their only business, and for making it widely known that there is no taxation, enrolment, or other liability incurred by giving full and true returns, and that there is no sectarian or political end to be served by the inquiry.

This excellent end is now well enough secured under the national law, and in several of the states. They have a still better method in Great Britain, where a system of registration of births, marriages, and deaths, has its districts and its agents under constant organization, and to which, once in ten years, the census can be assigned, without creating new offices. In Sweden, where a system of registration, including also a record of change of residence, is in charge of the parish-clerks, they take a census whenever they choose to post the books, without any special inquiries being made, more than what these records contain.

In the national census before 1850,— in New York before 1855, —and in some of the states still, each family had one line upon the blanks; and the number of persons of different ages, sexes and color, were entered in columns provided. The limit of classification was of course restricted to these columns; and, although the totals of each class were easily obtained by adding, the results were meagre and unsatisfactory.

The change that allowed a line for each name, one column for the exact age, and other columns for native country, profession or occupation, etc., while it simplified the labor of taking, allowed ample field for classification; and it made it necessary to employ a large force of clerks, in a central office, for the reduction of the returns for publication.

By a method now generally used in Europe, the census is taken upon "householders' schedules," which are distributed one to each family, some days beforehand, filled out by the head of the family, and collected upon *one day*. The only instance in which this has been done in the United States, within my knowledge, was in the

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District of Columbia, upon the eleventh of November, 1867. This census was taken by the metropolitan police, under my own direction, and with entire success. It was attempted in the city of Baltimore some months afterward, and failed, apparently from want of proper management on the part of those in charge.

For all kinds of official inquiries, relating to business, as well as to personal statistics, I think the true and proper method is, by means of *special blanks*, carefully prepared, simple, and fully explained. These should be distributed some little time beforehand, and should be taken up, if not in one day, within a short period of time, but with reference to a given day. The chief difficulty to be encountered is the illiteracy of those who should fill the blanks; but in the District of Columbia, which in 1867 contained a large number of colored families, but recently freed from slavery, the blanks had been, in almost every case, filled out by some one to whom they had been carried.

In following our subject, the "Methods of Statistics," we may notice some points in the condensation and arrangement of facts that may be of interest.

With a vast amount of information before us, as, for example, in the returns of a census, let us consider what is to be done, and how it can be done with the least labor and greatest certainty. After inspection to make sure that the work is all together, in proper order and condition, it will be found that several distinct operations are necessary, in preparing the results for the press. Columns of figures must of course be added, and carefully revised. As the totals of several sheets will often be consolidated into one sum, it is best to use spare sheets of the same schedule for entering the totals of pages, so that these partial totals can be easily combined. It is always a good practice, where long columns, of many figures in each, are to be entered in adding, to provide paper with narrow vertical ruling, that shall allow of but one figure in a space. In cases where the first two or three right-hand characters are generally ciphers, they may be left out altogether, the significant figures only being entered in their proper places. It saves a little time and labor, and does not lead to error.

Where a great amount of statistical material is reported,— as, for example, the names in a census,— the blanks should always be plainly divided by horizontal and vertical lines, *printed in*

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preference to ruled. The horizontal lines should be numbered from the top downward, upon both margins. This numbering is the more important where an entry is carried across to another page. Each line should contain but one entry, and there should be, if possible, no blank lines except at the end. Then, with a little multiplication-table at hand, showing the number of lines in a full sheet, and for each number up to the highest that are likely to be found in a return, the totals can be rapidly and accurately ascertained, as follows: The number of sheets are first counted on the back edge, and the number of entries they should contain, if full, are set down. Then, by glancing over each page separately, it is easy to notice whether there are any lines with two entries, any blanks, or any lines in excess. The deficiencies are set down in one column, the excess in another, and their difference is added or subtracted, as the case may require, when the true sum is at once found. This operation, which is the first thing done, should be repeated by another person; and, when found to agree, it should be kept as a test-number for verifying the accuracy of much of the work that is to follow. In measuring parts of pages, a scale made of a strip from the margin of a blank schedule, and pasted upon a card, will save all labor of counting.

In statistical labors, where the same returns afford material for a considerable number of deductions,— as, for example, the population sheet of the census,— it is generally best to take *one thing at a time.* Thus, the ages, professions, nativities, civil condition, etc., should be taken by separate operations, and not two or more at once. There is not, however, the least need of confusion in keeping the subdivisions of these subjects, in two or four classes, — as, for example, ages by sex and color,— by a simple arrangement of heavy and light horizontal lines upon the tally-sheet, and a little practice in its use.

There is much to be gained, both in time and accuracy, by a proper arrangement of a tally-sheet. The grouping together of tally-marks, by making four down and one across, so as to divide the work into groups of fives, is so natural and obvious a method, that few who have had occasion for this kind of work could have failed to adopt it. By an arrangement which I have used to a large extent in census work, I have had my tally-sheets printed off into squares, so that each compartment should receive one group of five, and no more. Then, by a series of numbers with a com-

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mon difference of five, printed across the top of the sheet, at the head of each vertical column, the number of tally-marks in a horizontal row can be known at once, by glancing along the vertical column containing the last full group of fives, to the number printed at the top, and then adding the marks in excess, but less than five, in the next compartment. This saves all counting, and a considerable amount of time. There is also an advantage, on account of the eyesight, in having the tally-sheets of some other color than white: a neutral tint might be best, but I have found common manilla paper answer every purpose.

Plans have been proposed for using cards of different sizes and colors, properly inscribed or numbered, as counters, for classifying a variety of facts, forming together a definite whole. By using colors, the eye becomes, without mental effort, a guide to the hand, in their distribution into piles or cells in a case; and, when the work is done, their number may be accurately known by weighing, or by measuring the height of each pile. Those of different sizes could be separated by mechanical devices, without handling, and a little practice, without liability to error.

It may be said generally, that the chief, indeed the only real, difficulty, in the preparation of statistical data, consists in getting the facts correctly. There is nothing in the operation of a central office that needs to involve error; or, if an error is committed, there should be no difficulty in tracing it to the clerk who is responsible for it. An efficient way to secure accuracy in work would be, to make a money-charge against the clerk who commits an error, to be paid to the one who finds it. I believe that something of the kind is done in some of the statistical offices in Europe, a class of revisers being employed, who are paid by the fines thus imposed upon the careless.

With respect to statistics obtained by circulars addressed to persons supposed to have the information desired, we have every grade of value, from good to good for nothing. The result depends upon many circumstances; as, for example, whether the person making the return is paid, or is under some obligation to, or expects some favor from, the person or office making the inquiry; whether the inquiry can be answered by reference to a record, or by some research more or less conveniently made, or is to be supplied from personal opinion, and a general knowledge of the subject; or, finally, whether the question can be answered by any

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thing better than a guess, by one who knows perhaps very little about it.

I would hold it to be the general rule, that where the inquiries are few and simple, exact as to their object, and, if they refer to a record, exact as to time and subject, and especially if they can be returned upon the same blank, and without expense for postage or otherwise, a very large percentage will be answered without a second application. A repeated call would probably bring a third or a half of the remainder; but there will be, now and then, one who will fail to reply, unless under official or personal obligation to do so.

We have thus far considered the dealing with statistics that have been gathered from the whole of a given field of inquiry; there are other methods that deserve notice, and the first of these is that "by samples." A portion of some whole is carefully studied, and the results obtained are deemed applicable to the entire field.

The French statistician Moreau de Jonnès has given some instances of this method, as applied in times past, by persons who had acquired eminence, and whose work gained confidence; and very properly asks, "What is such work worth?"

Vauban, distinguished as a military engineer, at the beginning of the eighteenth century, wishing to know the agricultural production of France, and the revenue it would yield, resorted to a method which would appear strange enough now, but still may be called ingenious. He attempted to reach his object by taking an exact account of the production of a square league, reckoning the arable land, vineyards, pastures, and woodlands, with their products in quantity and value; and then, by the simple "rule of three," he said, "as 1 is to 25,000, so is the result to the whole of France."

The English agriculturist, Arthur Young, sought to ascertain the proportions of meadow-land, mountains, and the like, in France, by cutting up a map by lines following these features of the surface, and weighing the parts.

In 1790 Lavoisier, distinguished in science, and for this reason consulted by the national assembly upon a question of imposts, found no existing data that applied to the internal resources of the country, until he himself supplied them, by a method that is now altogether neglected in statistical researches. He proposed to as-

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certain the number of ploughs in the country, and from this to calculate the quantities, production, and consumption of agricultural crops.

In 1784 M. Necker, the distinguished statesman, deduced the population of France from an assumed percentage of the birth-rate; and this was taken for a census!

But coming down to a much later period, we find a remarkable application of the law of induction in a work upon the industries of France, by the minister Chaptal. He presents agricultural tables, which have been received with great confidence, since they bear the appearance of official statistics, and were executed under the Empire. His tables are found to have been computed, without acknowledgment, from a statement addressed by M. Hennet, director of the cadastral survey, in 1817, at a time when not more than a seventh part of this work had been finished. The other six-sevenths were obtained by a simple multiplication of the finished part.

Many years ago, a "distinguished statistician" published, with great apparent precision, the yield of potatoes in France. There had been no official inventory taken; but when one came to be made, some time afterwards, it was found that this deduction had been obtained by multiplying the yield of a single commune by 37,000, the number of communes in France.

These examples might be multiplied indefinitely; and we need not cross the Atlantic, nor go far back in time, to find them. There is scarcely a day, but that we see passing through the newspapers, estimates, deductions, and statements, that have no more solid foundation than those we have cited. Nevertheless, we must not wholly disregard the inductive method in statistics: there are many cases in which we can get nothing else.

The chemist must analyze the soils and the ores from samples. In every operation of testing the quality and the value of any commodity whatever, we must select from the material before us what appears to be the average quality. And so of statistics generally: if there is no actual and general inventory made, we must collect from what is deemed a fair average, and, from these data, obtain such conclusions as they afford. The result in this, as in everything, will depend upon the intelligence and honesty of the person who makes the estimate, the extent of his opportunities, his experience, and his skill.

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Returning to the field of exact statistics, we may remark, that we can never have an accurate census of the population until we have a thorough and uniform registration of births, marriages, and deaths; a measure which this Association undertook to promote, more than a quarter of a century ago, but which has not made successful progress.

We cannot have a faithful statement of the industries, without a record kept of the production, the consumption, and the cost of operation. This is already done by most of the important ones, as an incident of business; but we lose the advantages by the hurried manner in which the official inquiries are made. Yet upon these returns we rely for all that is collectively known about them.

It follows, that, until we can realize these desirable features, the best we can expect is, to afford more time for previous preparation, by submitting beforehand the questions that are to be answered; which can only be done by the aid of "householders' schedules" for population, on "special blanks" for each of the industries, or other subjects, that come within the range of inquiry.

It was my intention to dwell at some length upon the illustration of statistical facts by graphic methods; but time will not permit, and opportunity for full preparation has not been found. For more than thirty years I have been accustomed to note down the principles involved in these methods, whenever, in the course of a wide and varied range of opportunity, a new one was found; and it has been with me a cherished intention to present the whole subject in a systematic form.

We may concisely state, that graphic illustrations, using lines, areas, or angular spaces, often supplemented by colors, may be employed for representing either —

1. *Quantities*, with or without reference to time.
2. *Time*, in recurring, interrupted, or progressive periods.
3. *Direction*, or *relative position;* and
4. *Intensity* or *force.*

In general, but two elements can be clearly presented at once; but by a skilful use of different colors, or kinds of lines, subjects of the same nature may be admirably compared, and the relations of cause and effect not only illustrated, but even discovered and proved. It is often admissible to introduce subjects having dissimilar notation, — as, for example, degrees of temperature, and

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height of barometer, — in the same drawing; but in these cases each must have its own scale, and, in a general way, every diagram must have a scale for every element of the subject that is represented, either expressed or implied.

Quantities may be shown either as they exist at certain periods of time, or as they form parts of a general total; and they may be presented so as to exhibit successive subdivisions, down to any desirable degree. If the divisions of a general total do not require subdivision, they may best be shown by angular spaces, as sectors, which together make up the whole of a circular area. If the divisions have some qualities in common, the shades of color may be of different intensity, significant of the degrees of quality that may exist. But where there are successive subdivisions, or *parts of parts* of a whole, there is no way so exact as by means of rectangular areas, which may also be shaded in different tints, as well as to separate them one from another as to show differences of intensity or degree.

In both of these methods, as well of angular spaces as of rectangular areas, we can only show quantities as they exist at a given point of time. We catch, as it were, the conditions, as does the light, the image in a camera. They admit of no such thing as motion or change; but these changes may often be strikingly represented by a series of diagrams, presenting the conditions at different periods of time.

Where time and quantity are combined, we have an easy and striking means of illustration; and in this the time may be in recurring periods, such as the hours of a day, or the months of a year, or it may be progressive, as in a series of years.

For the recurring periods, I think there is nothing so convenient and instructive as the *circle*, in which the quantities are measured along the radii, from the centre as their base. The entire radius may sometimes represent the whole of that of which these partial measurements are a part.

For a progressive series, the ordinates representing quantities should be measured from a level base-line, and the scale of graduation shown upon the side margin, while the time is measured from left to right by a scale along the upper margin.

For a simple comparison, a series of bars or lines, measured from a common base, and either horizontal or vertical, is a convenient

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and striking mode of illustration, and has now come into very common use.

A rectangular area, with parallel divisions, amounts to the same thing as a line; but with this difference, that a secondary subdivision may sometimes be represented with great effect.

Having thus stated some points in reference to graphic illustrations upon a true principle, I should not leave the subject without word of censure for some that are false. I will specify, particularly, such as attempt to represent comparative quantities by *concentric figures*, such as circles or squares. The eye has, in these cases, no means of just comparison; and they are very apt to mislead, where they are intended to instruct.

The same objection may be made against similar geometrical solids; for, although they may be literally true, their contents being to each other as the cubes of similar lines, the eye does not readily see the difference. It would be better, in such cases, to use cylinders or prisms of the same base but proportioned in length to the quantities that they represent.

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ADDRESS[1]

BY

MR. EZEKIEL B. ELLIOTT.

VICE PRESIDENT, SECTION I.

[*Delivered at the Montreal Meeting*, 1882.]

THE SECTION OF ECONOMIC SCIENCE AND STATISTICS: ITS SCOPE AND ITS LIMITS.

TWENTY-FIVE years ago, in 1857, at the meeting of the American Association for the Advancement of Science held in Montreal, I had the honor of presenting a paper involving elaborate computations showing the law of mortality in the state of Massachusetts, United States, with corresponding tables of annuities and other monetary values, and comparing the results with those of other communities; a paper which would probably have been assigned to, and read before this section had this section then existed.

The British Association for the Advancement of Science, after the plan of which this Association was in a large part formed, was organized in 1831. Its section of Statistics was formed two years later in 1833, and the Section of *Economic Science and Statistics*, was established in 1856, 23 years later.

With us, the Section of Economic Science and Statistics was formally established in 1881 and is put into effective operation at the present session, in 1882.

Papers of a character entitling them to be read before this Section have hitherto in general been assigned to section A, that of Mathematics and Physics.

[1] The author was unable to prepare the address in time for publication in the Montreal Proceedings. It is therefore printed in the present volume by vote of the Standing Committee.— ED.

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The methods of investigation employed in considering these subjects were more closely allied to the methods of section A, Mathematics and Physics — than to those of Section B, Geology and kindred sciences.

Economic — pertaining to man and his welfare; man in his domestic and social relations; man, what he is, his property or what he controls, and his surroundings, over which he has little or no control — like climate, and the changes of the weather and of the seasons.

Economic Science. — The subjects which properly pertain to this section are scientific not speculative. The section is to deal with facts, known facts; not scattered, heterogeneous, unrelated facts, but known facts reduced to order.

Statistics. — The subjects considered must be truly scientific, that is, knowledge reduced to order, pertaining to the interests of man, either singly, as an individual, or associated in communities, or related by some common law.

The subjects to be submitted to this Section are not exclusively those which belong to economic science, but may pertain to any facts to which the application of statistical methods are peculiarly adapted, to facts susceptible of numerical expression and which may be arranged in tables; to any facts susceptible of such arrangements and groupings that the numerical laws which underlie may be elicited: numerical laws based upon the past, and which will afford facilities for confident and trustworthy predictions as to the future, — for example, the construction of life and annuity tables, correctly deduced from authentic records of population and deaths.

Among statistical methods useful and necessary to the eliciting of the more recondite and higher laws and results, the mathematical doctrine of probabilities plays an important part. It may be deemed desirable in the not very distant future, for this section to create a sub-section for the special consideration of the subjects and processes to which the mathematical doctrine of probabilities now specially applies.

Mr. Quetelet, the eminent Belgian astronomer and statist, whose acquaintance it was my good fortune to make at Berlin, in 1863, on the occasion of the convening in that city of the fifth session of the International Statistical Congress, there said to me that he

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had made an effort at a previous session of the congress, in London, to establish a section to be devoted to the application of the mathematical doctrine of probabilities to statistics—but that he had failed, that he had been making a like effort at the session in Berlin, then holding, but should fail, much to his regret, the number of trained mathematicians among the statisticians assembled, being small,—but that he was confident in his hope that at the next session of the International Statistical Congress he should succeed.

The next session of that congress was held two years later in Florence, Italy, and Mr. Quetelet did succeed; such a section was there formed, and has since continued a feature of that very important and useful organization.

A wide range of subjects properly belongs to this section of our Association — a section destined, I doubt not, to be one of great importance, and to be the promoter of practically useful results not merely to America, but to the entire world, — for the valuable results of economic and statistical inquiries are not limited to the people who foster them, but are as far-reaching as humanity itself.

This Section, as has been mentioned, embraces the consideration of a vast extent of special subjects relating to man, his nature, his possessions, and his surroundings of all kinds, including facts tending to show the condition and changing condition of communities, races, individuals, and interests.

An early and continuous knowledge of the condition and changes of condition of a community, and a prompt digest and publication of the facts, may tend greatly to its welfare, — not merely in time of peace and prosperity, but also and especially in time of war and of epidemics. The varying state of the community, or interest in question, may thus be brought clearly to the attention of the authorities and the people, so that evils as they arise may be instantly known and suppressed.

We may note as within the province of the consideration of this Section, units of measurement in general, as of longitude, time, space, force and energy, and other constants of nature and art.

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PAPERS READ.

The Increase of the Colored Population of the United States.

By Calvin S. Mixter of U. S. Census Office, Washington, D. C.

Since the publication of the statistics of the population of the United States as ascertained from the returns of the census of 1880, it has been frequently asserted that they indicated very clearly that the colored people of this country are increasing more rapidly in their present state of freedom, than they did when in slavery. While it is true that the census returns of 1880 when compared with those of 1870, seem to justify this statement, it is nevertheless true that there is reason to doubt the accuracy of the census of 1870, in this respect. When the general results of the tenth census, so far as they related to population, were published in the summer and fall of 1880, it was charged by numerous influential journals that the returns of the colored population of the southern states, especially those relating to South Carolina and Mississippi, exhibited so large an increase over those of 1870, that they were more or less fraudulent. This was reiterated so frequently that the Superintendent of Census, after consultation with the Secretary of the Interior and the President, sent two capable officials from his office in September, 1880, to investigate this matter, the results of which were embodied in an official report to the Secretary of the Interior, dated October 5, 1880, wherein he expressed the opinion that they "satisfactorily settle the question as to the fairness of the tenth census in South Carolina."

Another and fuller investigation was made by five other officials detailed from the Census office, in November, 1880, which continued for nearly two months and led "to the conclusion that the census of South Carolina was correctly taken."

These reports may be found in Appendix C, pages liv to lxxvi of the Introduction to the Compendium of the Tenth Census of the United States, 1880. An investigation was also made in Mississippi, which resulted in like manner.

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The reports relative to South Carolina give details as to the manner in which the investigations were conducted, and also as to the course pursued in 1870, by the enumerators who took the ninth census in certain localities, which will account for some of the principal defects in their returns. "The universal testimony obtained is that (in 1870) the names were obtained at court sessions, at musters and other like occasions, where considerable portions of the population were brought together, and not by a house-to-house canvass."

The following table exhibits the returns of the colored population, *i. e.*, persons of African descent, in 1880, 1870, 1860, 1850 and 1840, according to the U. S. Census reports, giving in detail the numbers reported from those states where the largest portion of such persons resided.

RESIDING IN	1880	1870.	1860.	1850.	1840.
Alabama	600,103	475,510	437,770	345,109	255,571
Arkansas	210,666	122,169	111,259	47,708	20,400
Delaware	26,442	22,794	21,627	20,363	19,524
District of Columbia	59.596	43,404	14,316	13,746	13,055
Florida	126,690	91,689	62,677	40,242	26,534
Georgia	725,133	545,142	465,698	384,613	283,697
Kentucky	271,451	222,210	236,167	220,992	189,573
Louisiana	483,655	364,210	350,373	262,271	193,954
Maryland	210,230	175,391	171,131	165,091	151,815
Mississippi	650,291	444,201	437,404	310,808	196,577
Missouri	145.350	118,071	118,503	90,040	59,814
North Carolina	531,277	391,650	361,522	316,011	268.549
South Carolina	604,332	415,814	412.320	393,944	335.314
Tennessee	403,151	322,331	283,019	245,881	188,583
Texas	393,384	253,475	182,921	58,558	....
Virginia	631,616	512,841	527,763	526,861	498,829
West Virginia	25,886	17,980	21,144	....	....
Total Southern states	6,099,253	4,538,882	4,215,614	3,442,238	2,701.791
Total all other states	481,540	341,127	226,216	196,570	171,857
Total United States	6,580,793	4,880,009	4,441,830	3,638,808	2,873,648

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An inspection of the foregoing table will show that in several states, and especially in Arkansas, Louisiana, Maryland, Mississippi and South Carolina, the population reported in 1870 is but slightly in excess of that reported in 1860, while that returned for 1880 shows a large increase over that given for 1870. In Kentucky, Missouri, Virginia and West Virginia there was a smaller population reported in 1870, than there was in 1860, but in these states there was also a considerable increase shown by the returns of 1880 over those of 1870.

The magnitude of this increase seems to indicate first, that, as has already been pointed out, the census of 1870, so far as relates to these states, was seriously defective, and, second, that the methods adopted in 1880 secured better and fuller returns than had been previously obtained. This result was mainly due to the fact that a much larger number of enumerators, in proportion to the population, was employed in 1880 than ever before, each of whom had a much smaller territory to canvass, and a better knowledge of the families and residences included within the limits of his district, and who, in the performance of his duties, was under the immediate direction and control of an official that had been selected and appointed for this purpose.

An examination of the rate of increase of these people since 1840, seems also to indicate defects in the numbers reported in the census of 1870, and further that they are not increasing any more rapidly now than formerly.

The rate of increase throughout the United States has been as as follows:

From 1840 to 1850 the rate was 26.6 per cent.
" 1850 to 1860 " " " 22.1 " "
" 1860 to 1870 " " " 9.9 " "
" 1870 to 1880 " " " 34.8 " "

When however periods of twenty years are taken the results are as follows:

From 1840 to 1860 the rate of increase was 54.6 per cent.
" 1860 to 1880 " " " " " 48.2 " "

which shows that there was a falling off of 6.4 per cent during the second period as compared with the previous period of twenty years.

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Owing to the disturbed state of affairs which prevailed during the war period from 1861 to 1865, it is impossible now to tell to what extent their increase was retarded, but as the adults among them now feel the burden of the support of their children as they did not in the days of slavery, and as they are not all as mindful of the rules of health as they should be, the probability seems to be, as the figures above given indicate, that their future increase will not be any greater than it has been in the past, but rather that it will not be as great.

ENHANCEMENT OF VALUES IN AGRICULTURE BY INCREASE OF NON-AGRICULTURAL POPULATION. By J. R. DODGE of Washington, D. C.

[ABSTRACT.]

THE paper begins by showing that national industry is prosperous in proportion to its diversity. The productions of agriculture would be unsalable if all the people were agricultural producers. A civilized nation, with the smallest proportion of non-agricultural workers in it, will be low in the scale of prosperity: with the largest proportion of non-agriculturists consistent with proper food-supply, the nation will be most prosperous. To a great extent, this is true also of the subdivisions of this country. It may be illustrated by the statistics of geographical sections that embrace groups of states, or by comparison of individual states. The author is even prepared to show, that, in a partly agricultural community, the increased employment of labor in industries that are non-agricultural stimulates improvement, compels higher culture, and makes the products of land, and the land itself, more valuable.

The states and territories are grouped for this comparison in four classes, which are thus designated: first class, having less than 30 per cent of agricultural workers; second, having over 30 and less than 50 per cent; third, having 50 and less than 70 per cent; fourth, having over 70 per cent, *i. e.*, being almost exclusively agricultural states.

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Classes.	No. of states and territories.	Acres.	Value.	Value per acre.	Per cent of workers in agriculture.
First class.....	15	77,250,742	$2,985,641,197	$38.65	18
Second class.....	13	112,321,257	3,430,915,767	30.55	42
Third class.....	13	237,873,040	3,218,108,970	13.53	58
Fourth class.....	6	108,636,796	562,430,842	5.18	77

In the first class are Massachusetts, Connecticut, New York, New Jersey, and Pennsylvania in the east, and some of the mining states and territories of the west, diverse in many points, yet alike in the fact of a large non-agricultural population. This class has only one-sixth of the population in agriculture. The fourth class consists of North and South Carolina, Georgia, Alabama, and Mississippi. It has nearly four-fifths engaged in agriculture, on lands worth only an average of $5.18 per acre. The states having only two-fifths in agriculture have farm-lands valued at $30.55 per acre, while those having nearly three-fifths in agriculture have lands valued at $13.03.

When individual states are compared, the results are equally marked. The author compares pairs of states,— Virginia and Pennsylvania, Kentucky and Ohio, Iowa and Illinois,— as follows:—

Items of comparison.	Virginia.	Pennsylvania.	Kentucky.	Ohio.	Iowa.	Illinois.
Per cent of agricultural workers.....	51.41	20.68	61.67	39.97	57.46	43.65
Value of farm lands per acre.....	$10.89	$49.30	$13.92	$45.97	$22.92	$31.87
Income of workers per annum.....	$180.00	$431.00	$199.00	$394.00	$448.00	$467.00

Similar computations give figures for the State of New York not widely different from those of Pennsylvania. The average value of farm-lands per acre in New Jersey is greater than in any other state, viz., $65.16: this is owing to a position between two great city markets, with proximity and easy access to both. Her per-

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centage of agricultural workers is only 14.92; their average annual wages are $501. It will be seen that the wages of agricultural labor are subject to the same law. If computed for the first table in this paper, the wages per month of the agricultural laborer would be found, for the first class of states, fully $25; for the second, nearly $25; for the third, less than $20; for the fourth, about $13.50.

An application of the same test to the value of annual production for each man engaged in agriculture brings equally interesting results in the following table:—

Classes.	No. engaged in agriculture.	Value of products of agriculture.	Value per capita.	Per cent of workers in agric.
First class	1,060,681	$484,770,797	$457	18
Second class	1,566,875	616,850,959	394	42
Third class	3,017,971	786,681,420	261	58
Fourth class	2,024,966	324,237,751	160	77

The states with less than 30 per cent in farm-labor realize nearly three times as much per man as those which have over 70 per cent in farm-work. In other words, one man in the first class realizes as much as the three men who are competing with each other, having little outlet for surplus production. Three brothers in Alabama, laboring through the year, get as much for their aggregate produce as one farmer receives in Pennsylvania, simply because that farmer has a brother engaged in manufacture and another in mining. It is because in one case there is a market for one product only, thousands of miles away: in the other, there are markets at every door.

It appears evident that the proportion between agricultural and non-agricultural population is a measure of the values of the land, of the production, and of the labor of the farm. These values are rapidly enhanced by the increase of non-agriculturists. This is the lesson of the most authentic statistics of our own and of other countries.

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BUILDING ASSOCIATIONS. By EDGAR FRISBY of Washington, D. C.

BUILDING Associations have a twofold object. 1. They answer the purpose of savings banks in enabling members to lay by certain sums monthly and receiving interest on the investment. 2. They enable persons of limited means to borrow money at a fair rate of interest by paying back the principal in small monthly instalments. It is proposed in this paper to see if we can discover any mathematical principles by which an association can be conducted that will make it equitable to both classes.

We shall assume that six per cent is a fair rate of interest, and allow a member to buy out on any month $200.00 or less from the association according to competition, for which he pays $1.00 interest monthly, and we will also suppose that he pays $1.00 monthly on the principal (this amount however may be varied); it will simply be paying interest monthly, and when the amount paid in on the principal (which must itself bear interest) is equal to the amount borrowed, the stock will be cancelled.

Let us take the most general case.

Let R = interest of $1.00 for one month (half a cent) and $1.005 = R =$ amount for one month.

We will suppose further that the member pays $1.00 per month for y months, then buys out at the maximum and pays afterwards $2.00 monthly; the value of his payments will be all the dollars he has paid in with interest, together with his prospective payments discounted, that is:

$$1+R+R^{2} \quad +R^{y-1}+2\left(R^{-1}+R^{-2}.....+R^{-n+y}\right)=200$$

by summing these series we obtain

$$R^{y}-1-2R^{-n+y}+2=200\,(R-1)=1 \text{ because } R=\tfrac{201}{200};$$

this reduced gives

$$R^{-n+y}\left(R^{n}-2\right)=0 \text{ or } R^{n}=2$$

This equation is entirely independent of y and herein lies the absolute equitability of the whole system. If the person bought out on the first month $y=1$, and he must pay six per cent compound

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interest if he bought at the maximum; if $y = n$ the person is an investor and receives six per cent; if y has any other value he receives six per cent until he buys out, and pays six per cent afterwards. This equation also shows that it is precisely as advantageous to buy in one month as another (on the supposition that money sells at a uniform rate) and also that all classes are equally benefited; and again, if a surplus of money is on hand and a member is drawn out, he has the option of taking the amount paid in with interest or an advance at the maximum rate. In the first case he receives six per cent interest, in the second case he pays six per cent interest.

If we solve the equation $R^{n} = 2$ we obtain $n = 138.9532$ which shows that \$200 borrowed can be paid up in about 139 months.

Example.— A person buys at the maximum and settles on the 101st month; we may suppose he is indebted to the association \$200 and credited with a compound annuity for 100 payments; or we may suppose he is indebted to the association \$200 with compound interest and credited with a compound annuity for 100 payments at \$2.00 per month.

In the first case we have

$$200 - \left(1 + R + R^{2} \ldots\ldots + R^{99}\right) = 200 - \frac{R^{100} - 1}{R - 1}$$
$$= \$200 - \$129.3366 = \$70.6634$$

In the second case we have

$$200\,R^{100} - 2\left(\frac{R^{100} - 1}{R - 1}\right) = 200\left(2 - R^{100}\right) = 200(.353317)$$

$= \$70.6634$ which is the amount of indebtedness; if, however, as is usual, he was only allowed simple interest on stock, he would have to pay not six but 6.6 per cent per annum.

If again the investor only received six per cent simple interest he may pay in not 139 months but somewhat more than 146 months, unless the association has wound up before. If a person pays \$1.00 monthly as interest and pays \$1.50 or \$2.00 on the principal, the case will be precisely the same as far as the equitability is concerned, but we should have $R^{n} = \frac{4}{3}$ or $\frac{3}{2}$ and the association would wind up more quickly; instead of 139 months we should

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have 102.4 and 81.3 months respectively. We have made all the computations by compound interest for two reasons. 1. Because the association invests the money monthly. 2. Because it is the only philosophical way, other methods leading to numerous fallacies; it is however usual to allow only simple interest on stock in settlement. Many methods may be devised to provide for running expenses. 1. Five per cent instead of six may be allowed to investors and in settlement, but if we consider the difference between the simple and compound interest and also the fact that money is generally advanced considerably below the maximum, I think we have a sufficient margin.

Suppose a building association starts with 2400 shares, cancels 200 every year and runs twelve years: the difference between the simple and compound interest will be over $10,600 in the twelve years, and this difference comes out of the shareholders very nearly in a proper ratio for the money invested. It is altogether more equitable even in this particular than the plan of taxing each share proportionately, independent of the time it has been in, or of the benefit to be received.

Life Insurance and Self Insurance, with illustrative Tables. By Elizur Wright of Boston, Mass.

[ABSTRACT.]

The intent of this paper is to show that whenever the net natural annual premiums by any table of mortality, covering the whole life or a long term of years, are commuted into a constant or level premium, the policy or contract involves, or implies, a banking accumulation, which diminishes, by so much, the risk borne by the company, and is therefore a SELF INSURANCE, causing the company to insure in any year only the difference between it and the face of the policy.[1]

[1] The paper was presented by its author in print, and copies may be had of him, by application to Box 109, Boston, Mass.

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The German Carp and its Introduction into the United States.

By Chas. W. Smiley of Washington, D. C.

[ABSTRACT.]

The German carp which has been introduced into the United States is the *Cyprinus carpio* of the family *Cyprinidæ*. There are three varieties: the scale, the most edible; the leather, the most prolific; and the mirror, which is intermediate between the others.

Although a few carp were brought from Holland in 1830, and put in a pond in Newburgh, New York, whence some were washed into the Hudson river, and produced no practical result, the introduction proper dates from May 26, 1877. At that time 345 carp were imported by the United States Fish Commission, under the direction of Professor S. F. Baird. The first distribution from these was of 6,300 fish in 1879, followed by 31,443 in 1880. During the past season 113,605 have been scattered in nearly every state of the Union.

The proper habitat of the carp is a pond of warm water with a muddy bottom, and some suitable aquatic plants. It does not prey upon other fishes or the higher forms of life, but feeds upon crustaceans, seeds, certain leaves of plants, etc. It may also be fed to advantage upon lettuce, cabbage leaves, grain, meal, bread, crackers, and in short, most anything suitable for chickens. Its regular growth depends largely upon its environment. If the climate is suitable, the food abundant, and its enemies scarce, its growth will be surprisingly rapid, reaching easily three pounds in one year or six pounds in two years. If food is scarce and the climate cold, the growth will be less rapid, and from two to four months will be spent in hibernation during which there will be no growth. This fish is consequently exceedingly well adapted to the Gulf States. It may be reared unquestionably in the northern states but more slowly. It is hardy enough to endure an intense degree of cold if the lowering of temperature be quite gradual. Carp usually spawn in cool latitudes when three years old, in temperate latitudes when two years old, and under the most favorable circumstances in southern Texas it is believed to have spawned at the age of one year. Among the enemies of the carp from which it should be protected are all kinds of carnivorous fish, frogs, snakes, turtles, terrapins, kingfishers, bitterns, cranes, her-

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ons, and fish hawks. A snake recently killed was found to contain twenty-five young carp and numerous undigested skeletons. A marsh hen was examined whose stomach contained thirty-eight young carp while in a night heron were found the heads of seventy-eight young carp. When about to spawn, one should separate the carp from all other animals in order to protect their eggs and the young until they reach such maturity as to be able to protect themselves. As food, carp is superior to all ordinary native fish such as buffalo, catfish, suckers, mudfish, croakus, mill roach, perch, sunfish, etc. Although by many who are cultivating them, it is declared equal to bass, trout, and shad, the Fish Commission makes no such claim. The flavor of the carp as of all other fish will be affected by its food and environment. If the water is polluted or the food offensive, the flesh of the carp will partake thereof. A carp thus reared should be removed to pure water for a week and kept without food, during which its system will be purified, and the flavor of its flesh immensely improved. Salting over night is also said to largely destroy the muddy taste when present. During, and for a time after spawning, the carp like all other fish, is soft and unsuitable to eat. Most complaints against carp have arisen from eating it at this period. The bones are not offensive as the flesh flakes off very nicely in the adult carp.

Between October and February of each year, young carp are distributed from Washington to those who have previously filed applications. Car loads are sent in the cars of the Commission which are suitably fitted for carrying all kinds of fish to central points. There they are either delivered to State Fish Commissioners for distribution, or are distributed by express to interior parts. The latter method is by means of a tin pail containing two quarts of water in which fifteen or twenty carp will survive from thirty-six to forty-eight hours and even longer if the water is changed meantime, always provided that water enough remains in the pail to cover the backs of the fish. The commissioners of several states undertook to propagate some of the first carp distributed by Professor Baird, and are now beginning to distribute the young which they have reared therefrom. Speculators have also entered the business, one of whose price-lists states that he can sell mirror carp, ten months old, at seventy-five dollars per hundred, and scale carp, ten months old at seventy dollars per hundred. Larger fish have

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been sometimes sold at five dollars a pair. A gratuitous distribution by the United States Commission has alone prevented prices becoming much larger.

The cultivation of fish is destined to become as important to American farmers and planters as that of cattle, poultry and fruit. The latter left to shift for themselves and to cope with nature come off second best, or succumb. If fish should receive the same care and attention, as to artificial propagation, and the improvement of varieties, the result would be equally gratifying. The practice in carp culture will teach some of the methods, and perhaps foster a taste for the culture of other fish. Every rural community is destined to have its fish ponds as abundantly as its poultry yards. Farmers will then have fresh fish, at will, no matter how remote from market. The carp may be tamed so as to come to a given place for its food at call, and if a shallow place or a suitable platform under the water be arranged, it can be induced even to come there and feed from the hands. It can then be captured easily. Success in carp culture, like success in any other useful enterprise, comes not of itself, but as a reward for careful attention.

OYSTER-FARMING IN CONNECTICUT. By HORACE C. HOVEY of Minneapolis, Minn.

[ABSTRACT.]

FOR many years oyster-dealers obtained their supplies from what are termed "natural beds." These are found of various dimensions, and usually exist in the brackish water at the mouths of rivers, such as the Quinnipiac and Housatonic, in Connecticut, and the Delaware, Susquehanna, and other streams emptying into the Atlantic Ocean. The early settlers of New England resorted to the natural beds without restriction, and regarded them as inexhaustible. Their implements were of the most primitive sort, such as shovels, rakes and the familiar oyster-tongs. Dredging by hand and by steam has been more recently introduced. The first law for the protection of oysters was enacted in Connecticut, in 1784, and remained substantially unchanged for the next seventy years. The necessity of a close season during spawning-time was

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recognized; but each town was left to make and enforce its own rules. When "the law was off," which was usually about the first of November, so vigorous was the assault upon the beds, that the more accessible of them were often scraped clean in a single day. It finally became evident to observing men that this process was ruining the natural beds. For various reasons the oysters deteriorated both in size and quality. This led to the importation of oysters from southern waters, which were found to fatten in the waters of Long Island Sound, although not propagating there. Capt. Eliphaz Gillette, sixty years ago, brought the first cargo of Virginia oysters to Fair Haven, Conn.; thus originating a trade that grew until, in 1858, there were 250 schooners engaged in it, with an average carrying capacity of 3,000 bushels each, and, according to De Broca, they handled annually 2,000,000 bushels of oysters. About that time (in 1856), Mr. D. D. Mallory opened his wholesale house in Detroit, and that was the beginning of the interior trade in shellfish in the United States. He subsequently fixed his headquarters in Baltimore, and, together with other men from Fair Haven and native Baltimoreans, worked up the national oyster trade. The immediate tendency of this, combined with other causes, was a steady decline in the business, so far as Connecticut was concerned. There was also a marked decline in the productiveness of the great natural beds of the Delaware and Chesapeake Bays. This made it desirable to introduce systematic oyster-culture to meet the constantly increasing demands of the market.

In this connection mention should be made of the brilliant experiments conducted by Prof. W. K. Brooks, Lieut. Francis Winslow, U. S. N., and others, who have tried, with some success to facilitate and regulate the propagation of the oyster, so as to enable the old and exhausted natural beds to be restocked.

My purpose, however, is to describe the more practical, and perfectly successful plan of oyster-farming; by which is meant the cultivation, on a large scale, of ground outside the natural beds. Let me first refer again to the preliminary experiments at transplanting southern oysters to northern waters. Within the last five years nearly seven-eighths of the oysters commercially handled at Fair Haven were what are known as Virginia plants. The process of transplanting was attended with risks and expense, but the profitable result was that oysters which were tough and taste-

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less while growing in still waters, became improved both in size and flavor by lying for a few months in running waters or on springy bottoms, like those at the mouths of the streams emptying into Long Island Sound. Then again, while the native beds, which are public property, are preyed upon by swarms of enemies, such as the skate, winkle, drill and star-fish, these pests may, to some extent at least, be removed from lots under the control of individuals. Hence originated a series of acts passed in successive years by the legislature of Connecticut, unlike those of any other state, except as they have been recently imitated, and that appear to be fragmentary and disjointed, until one understands the emergencies they were intended to meet. The popular feeling was that whatever grew in the sea was common property. Planted beds were treated as if they were natural and open to all. No title could be held to submarine property. So far as can be learned, the first artificial bed of natives was cultivated by Mr. Willard Mallory, about fifty years ago, on the West Haven flats. Having raised several hundred bushels, his unscrupulous neighbors gathered in the harvest by night for themselves. This was regarded merely as a rough joke, for which there was no legal redress. The first step toward a better state of things was the legal recognition of riparian rights, by which land-owners along the shore were protected in planting as far as low-water mark; or in estuaries, as far as the channel of the stream. In 1855 a law was passed allowing towns to designate to each applicant two acres for planting oysters. In 1875 growers were allowed to buy more than two acres, provided they did not infringe on natural beds. This made oyster-farming possible.

It may be stated here that Connecticut is probably the only state that grants permanent deeds of this description. The system in Massachusetts is to license oystermen for a limited term; and the regulation of the business is mainly left in the hands of the selectmen of the several towns. In Rhode Island, oyster grounds may be rented at the rate of $10.00 per acre for a limited term. New York, Virginia and Maryland, while enjoying vast and lucrative oyster grounds, grant no permanent deeds, but restrict growers to small tracts for immediate use, and not exceeding five acres to one citizen. Delaware, since 1881, leases ground in deep water, on terms favorable to oyster-farming.

In 1878 a special commission was appointed by the states of

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New York and Connecticut to fix the boundary line running through the Sound. This division, ratified by the U. S. Congress, and geographically fair, left to Connecticut a marine territory of 565 square miles, including nearly all the best oyster-ground in the Sound. The average depth over the whole area, not including bays and estuaries, does not exceed seventy-two feet. The portion west of Norwalk averages about sixty feet; east of Norwalk to the Connecticut river is a strip sixty miles long by four miles wide, that averages a depth of but forty-two feet. To the south of Stratford is a plateau including sixteen square miles, only twenty to twenty-five feet deep, and entirely free from mud and slime.

In 1879, an oyster-commission was appointed to inquire into all the bearings of the oyster question, and devise a uniform system of laws. It was found that the available flats and shallows belonging to the several towns had been taken up, while a few bolder spirits had ventured to stake out lots in the deeper waters. Conflicting grants had, in some cases been given, and there was serious danger of matters becoming entangled in inextricable confusion. Hence, in April 1881, the state conferred on her Fish Commissioners the power to act also as Shellfish Commissioners, and to adopt a plan for utilizing and distributing the state oyster-grounds. They ran an inshore line from point to point, within which the grounds should remain under town control. The most accurate survey possible was next made of the territory lying between the inshore line and the state line, involving 118 triangulations, the triangulation sides varying from one to seventeen miles in length. This work is so exact that the Commission are able to define promptly the limits of any oyster-ground within the state jurisdiction. Mr. Bogart, the surveyor, informs me that the entire area includes 325,000 acres, of which about 15,000 acres are rocks and reefs, while a large portion, perhaps two-thirds, of the remainder is fit for oyster-cultivation with modern appliances. The entire acreage thus far apportioned is 100,000 acres.

In order to appreciate this rapid stride, one has only to reflect that, according to the Census of 1880, the total acreage of Connecticut oyster-grounds included but 6,934 acres. And now, three years later there are several individual oyster-farms each exceeding that large area! The largest of all belongs to Mr. Henry C. Rowe, who holds deeds for more than 18,000 acres. The whole number of oyster-farms staked out during the last three years is

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376, varying in size from two acres to the large tracts already mentioned.

The natural beds have also been surveyed. Their area does not exceed 5,000 acres in all. They are free to all citizens of the state, and are now chiefly resorted to for what are termed "seed-oysters," *i. e.*, those not more than two years old, which age is regarded as the best for transplanting. All other ground is for sale at the nominal price of one dollar an acre, with ten cents added to cover expense of designation. If not cultivated within the next five years, it is forfeited. If found, on trial, unsuitable for cultivation, the money is refunded. Farms are taxed, but the oysters produced are exempt.

The oyster-farmer having secured his ground next cuts it up into smaller lots in order to till it systematically. This is done, of course, by buoys in deep water, and by stakes in shallow. Next he carefully examines the nature of the bottom. Much of the bottom of the Sound is smooth, hard sand. Here and there is a patch of gravel, cluster of rocks, or a bit of muddy ground. It is to be noticed, however, that there are no oysters, and his problem is to make them grow. Oysters cast their spawn at various times between March and November, but mostly in July and August. Each female contains from ten to sixty million eggs. Few of them, proportionally, mature. The young oysters are for a few days free floaters, and then they are ready to be attached to any clean substance that may offer. The plan has been tried in the vicinity of Groton, in the Pequonnock river, of thrusting down small trees, white birch being preferred, letting them lean at an angle of forty-five degrees, sloping with the current. The young oysters set on these very readily. They are harvested when mature by the simple process of pulling up the bushes and stripping off their singular fruit. As many as 1,000 bushels of superior oysters have been gathered from an acre in a single season! There are objections, however, to bush cultivation. Oysters will set on almost any object that they come in contact with. Clean gravel is preferable to anything else, where it can be had. Old, dry shells are generally used for the purpose; and the fact is significant that, whereas they were to be had gratis for road-making, five years ago, they now command from four to eight cents a bushel. That is as much as veteran dealers used to pay for live oysters by the cargo! Shelling begins in June and continues into August. In shoal water

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spawning is earlier than in deep, owing to difference in temperature. Planters average about 300 bushels of shells to an acre, and scatter among them spawners in the proportion of 100 bushels of spawners to 1,000 of shells. Some idea may be had of the magnitude of the business from the fact that Mr. H. C. Rowe has planted this season more than 300,000 bushels of shells, at a cost of $25,000! Crops correspond to the planting. The Commissioners say, "One cultivator alone looks for no less than one million bushels of marketable natives from his own ground this year!" Yet there are possible reverses. One firm lost last year from a single bed $20,000 worth of oysters by the ravages of starfish. Owing to the same cause, as I am informed by Mr. Bogart, nearly all the natural beds have proved a failure this year, so far as large specimens are concerned.

All the large farms are now cultivated by means of steam. This advance on the old methods was led by Capt. Decker, of Norwalk, who put a donkey-engine into his sloop, the Early Bird, for the purpose of turning the drums on which the dredge-lines were hauled. That was in 1867. The boat is still in service, rigged as a steamer, hauling two dredges, and having a capacity of 200 bushels. Mr. W. F. Lockwood constructed the first steamer built expressly for the oyster business. Mr. Joshua Leviness, of City Island, endeavored to introduce the same improvement into the New York waters, but a law was passed forbidding steam-dredging in that state. Efforts to secure similar prohibitory action in Connecticut were unsuccessful. There are now thirty or more steam-dredgers at work there, with a carrying capacity of from 200 to 2,000 bushels each. The largest catch made in one day was by the steamer Smith Brothers, namely, 1522 bushels in six hours in water forty feet deep.

The traffic in seed-oysters (two-years old) has lately become very lucrative. They go to New York, Rhode Island and elsewhere outside the state, and command from fifty cents to a dollar a bushel. A single firm sold this year about 100,000 bushels. They are used for planting. The third year of an oyster's life witnesses its most rapid growth. I have known instances in which seed-oysters have increased in six months from a length of two inches to a length of five inches! Hence buyers, as well as sellers find it a profitable business.

Comparatively small oysters are preferred for the foreign market;

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while the demand for large ones is constantly increasing in this country. Four grades only are recognized by the oyster growers: viz., "cullentines" which are from two to three years old; "culls," three years old; "boxes," from four to six years old; "extras," from five to seven years old. Those over seven years of age are generally tough and undesirable. The oldest specimen I have seen was estimated to be twenty-five years of age. Culls and cullentines are shucked and canned. Boxes and extras are shipped in the shell in barrels tightly packed. The export trade is in its infancy; yet there were 500,000 bushels of oysters in the shell shipped from Connecticut to Europe last year.

NOTE:—In connection with the above paper, Mr. Hovey exhibited specimens of living oysters varying from one day up to twenty-five years of age. He also, by the courtesy of the Shell-fish Commission, exhibited their official chart of the oyster-grounds of Long Island Sound.

IMPROVED METHOD OF SPRAYING TREES FOR PROTECTION AGAINST INSECTS. By C. V. RILEY of Washington, D. C.

[ABSTRACT.]

THE paper gave a summary of results obtained from experiments made during the past two years at the U. S. department of agriculture. An ordinary barrel is used as a reservoir, in which is inserted a force-pump with automatic stirrer. A long rubber hose extends from the pump, and is attached to the spraying apparatus. The nozzle has been called the cyclone or eddy nozzle; its action carries out new principles of spraying. It is a shallow, circular, metal chamber, with two flat sides, in the centre of one of which is a small circular outlet. The fluid is forced into this chamber tangentially, producing rapid rotation, and a spray which is easily regulated from a mist scarcely visible to a strong shower according to the size and form of the outlet. This nozzle is adjusted to the end of a bamboo rod (of varying length, according to requirement), through which the rubber hose has been passed; or several nozzles may be attached, in different positions, to the side of a stiff metal

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tube, sufficiently slender to be handled by the operator, and thrust among the branches of the tree. The metal tube is made in sections that screw on to each other. By these means, trees from twenty to thirty feet high can be rapidly sprayed without the use of a ladder. The substances used are either London purple, one-half pound, and flour, one pound, in from forty to fifty gallons of water; or Paris green, one pound, to the same amount of flour and water; or petroleum emulsions made as the writer indicated at the last meeting of the association.

CABLE-CARS FOR CITY PASSENGER TRAFFIC. By E. T. COX of New Harmony, Ind.

[ABSTRACT.]

THIS paper described the success of the cable system as a substitute for horse-cars; and urges its general adoption, on the ground of its convenience and comfort to humanity, and the diminution of suffering to the horse. Numerous statistics were given relating to the number of horses now employed.

A NEW SYSTEM FOR THE TREATMENT AND REMOVAL OF SEWER GAS, THE VENTILATION OF BUILDINGS, AND UTILIZATION OF HEAT. By T. E. JEFFERSON of Hudson, Wis

[ABSTRACT.]

THIS system chiefly consists in making sewers approximately air tight, by sealing the sewer inlets, so as to admit sewage but exclude the air, and by making suitable pipe connection between sewers and buildings and different heating apparatus, arranged to permit the inflow of atmosphere, and the products of combustion into the sewer, and at the same time prevent the backflow of gas, when by the connection of a powerful suction apparatus with the

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sewer near its outlet, the removal of sewer gas, and smoke from furnaces and fires and the thorough ventilation of buildings, is positively effected and regulated as desired. By employing mechanical force for creating draft for fires, the large per cent of heat heretofore required for this purpose is retained, effecting a corresponding saving in the consumption of fuel. The main portion of these important discoveries, including the removal of sewer gas and the positive means for ventilating buildings and cities, and carrying the vitiated atmosphere and poisonous vapors away from contact with the people, were recently made by Hon. John Comstock of Hudson, Wis., and first introduced in one of the districts of Paris, France, the present year, where its great utility and success are fully demonstrated and established.

"HEALTH FOOD." By STEPHEN S. HAIGHT of West Farms, New York, N. Y.

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EXECUTIVE PROCEEDINGS.

REPORT OF GENERAL SECRETARY.

THE THIRTY-SECOND ANNUAL MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

IN accordance with the action taken at the Montreal meeting in 1882, the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE met in GENERAL SESSION in the Chapel of the State University of Minnesota, in East Minneapolis, at 10 A. M., August 15, 1883; the retiring President, Dr. J. W. DAWSON, of Montreal, in the chair.

On calling the meeting to order, Dr. Dawson completed his official duty as President by calling to the chair the President-elect, Prof. CHARLES A. YOUNG of Princeton, N. J.

On taking the chair, Prof. Young said:

Ladies and Gentlemen of the Association:— I should certainly be sadly wanting in all proper appreciation of the circumstances, if, in receiving this chair from my distinguished predecessor, I did not express my sincere and hearty thanks for the high favor you have conferred in electing me to its occupancy.

In taking it, I can only promise what is in my power — not high skill in the conduct of business, nor any thorough knowledge of parliamentary form and practice, but only an honest and patient fidelity to your interests, and strict impartiality. I must rely to a great extent upon the assistance of our permanent officers, and upon your own kind indulgence.

We are met here for an important purpose — to do what we can, in the short interval allotted us, for the "Advancement of Science." To this everything else should be kept subsidiary. Not only in the meetings of our sections, the reading of our papers, and our discussions, but in all our more agreeable occupations here, let us keep an eye single to this aim, and strive so to carry ourselves and so to use our opportunities, that we, and all our friends here, shall be stimulated and encouraged in the honest pursuit of truth.

Prayer was then offered by Bishop CYRUS D. FOSS of Minneapolis.

The Hon. GEORGE A. PILLSBURY, chairman of the Local Committee, then introduced the Hon. LUCIUS F. HUBBARD, Governor of the state of Minnesota, who delivered the following address of welcome on behalf of the state.

Mr. President, Ladies and Gentlemen of the American Association for the Advancement of Science:— I trust that you have already realized the

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fact that you are cordially welcome in our midst. We assume that one of the first impressions that a stranger forms here is that he has come among a people that are glad to greet him, and expect him to make himself thoroughly at home. Such is our hope in your case, and we shall regard it with disappointment if it shall be necessary to make a formal tender of our hospitality in order to make you feel that it is freely yours. We welcome you to our state, and we feel honored by your presence. We recognize in your association a body of ladies and gentlemen who are devoting their talents and energies to the advancement of the greatest material interests of our race, and whose efforts should command the cordial recognition and reward which they deserve. The achievements of science in this generation are an epoch in the history of the world. These achievements have wrought essential changes in the material condition of man. We hardly approach to a realization of the latest scientific wonder before we become almost transfixed by some new surprise. Scientific investigation has proven a most inviting field for the labor, and commands the service of the brightest intellect and genius of our time. At the same time it has returned a rich harvest to the laborer and has dispensed untold and unmeasured benefits to mankind. We expect to derive much advantage from your presence at this meeting. The great northwest offers, no doubt, a grand field for scientific inquiry, and will furnish ample material to illustrate your investigations. We shall seek to profit by your deliberations here in common with the country at large, which will regard your proceedings with an interest commensurate with their importance.

The Hon. A. A. AMES, Mayor of Minneapolis, was then introduced and in the following words welcomed the association on behalf of the city:

Ladies and Gentlemen of the American Association for the Advancement of Science:—You have honored the city of Minneapolis by selecting it as the place for this your thirty-second meeting. Our citizens are not unmindful of the honor conferred. On behalf of our people, I bid you welcome, and I take pleasure in extending to each of you the freedom and hospitality of our city. You will find them as broad as our streets and as cordial and courteous as men with western ideas can make them. You must not suppose, gentlemen, in looking over our city casually, that we are devoid of science — that we are wholly given up to a chase after trade dollars. Here, in the West, we combine business with pleasure. Many of us took Horace Greeley's advice, and here we are grown up with the country. Our science consists of means devised for tilling the vast and fertile prairies which surround us, levelling the gigantic pines of the north, and building mammoth mills, workshops and blocks, and constructing beautiful homes. You find no ancient castles or fortresses here: everything wears a modern air. We, as a city, have sprung into existence as if by magic and we stand before the world one of the wonders of the nineteenth century. Within my own observation Minneapolis has grown from a wild waste inhabited by Indians and wolves, to be a city of 100,000 people. This comes of western energy and western thrift. Here you

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have a problem not solved by any known scientific formula. It is a case which, like the great trees of California and the wonders of the Yellowstone region, still remains to invite the theories of scientists.

Again I welcome you to our city and homes. You will find the latch strings hanging out, and plenty of hospitality and cheer within. If, among your other scientific researches, you will solve and make plain to us the science of local government, you will confer a lasting favor, and for your labor you shall not go unrewarded. Between your hours of research we invite you to see how we have harnessed up old St. Anthony's Falls and utilized the great water power of the Mississippi river; to go through our mills, manufactories and workshops and see the results of skilled labor and improved machinery; to visit our beautiful lakes and pleasant resorts and view the most gorgeous scenery ever spread before mortal eyes; to see Minneapolis and her people as they exist in their primitive sereneness. Gentlemen, I bid you make yourselves at home among us.

In behalf of the Local Committee, Col. Wm. W. Folwell, President of the State University, made the following address:

Mr. President and Gentlemen of the American Association for the Advancement of Science:—His Honor the Mayor, and His Excellency the Governor of the state, have bidden you welcome to our city and commonwealth in due form, with fitting words. You are accordingly welcomed. Yet, I am commissioned to offer you still further welcome. This surprises, perhaps amuses you. It did, I confess puzzle me at first not a little, to guess what my townsmen intended when they charged me with this duty. A little reflection resolved my doubts and convinced me that my colleagues of the local committee, acting for our good people of Minneapolis, knew what they were about. The chief magistrates of the city and state would give you as in duty bound, their formal official salutation, which you, being all Pickwickians, might interpret in the Pickwickian sense: as the prudent traveller interprets that oriental courtesy which lays at his feet the house, equipage and whole wealth of his host. Our people wished more than this; they meant more than to open to you the doors of the capitol, the city hall, the police court, the waterworks and other public institutions. So they send one of their number in no official capacity or relation at all, simply as one of themselves, to clap atop of the most polite yet official greetings of the Mayor and His Excellency, a hearty, informal, old fashioned "how d'ye do," and beg you to "make yourselves at home with us." Accept then, I beg you, the greeting and welcome of the people of Minneapolis.

I think it is further expected of me that I attempt some explanation, I need not say apology, of the condition of things you must already have observed in our city. Look where you will, you see the streets blocked by piles of building material and the throng of loaded wagons. Here are sewers in construction, there pavements. Hotels, stores, manufactories, dwellings, are rising in all quarters. Vast railway improvements are in progress to meet the demands of a commerce already colossal. You

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wonder, I am sure, that there should be any city at all, where so lately the terrible Dakotas were masters of the soil. We Minneapolitans live in a chronic state of amazement, as wealth and population flow in upon us, as suburb after suburb springs up, and vast structures lift themselves in our central precincts. As for population, most of us have lost the count, but no one, I think, would place our number at the next census in 1885 below one hundred thousand souls.

For this magnificent natural development the causes are not far to seek. On the part of nature they are threefold: first, a vast area of arable land of great natural fertility, unincumbered by forests, ready for the plough, lying behind us in our own state, in Dakota and the territories beyond; second, the great pine forests, covering the broad plateau between us and the Canadian line, whence flow the Mississippi and his upper tributaries; third, the Falls of St. Anthony furnishing, it is estimated, the largest available water power of the world. All these, however, would avail little but for the intelligence, foresight, enterprise, faith and trust of men. It is these human qualities which have brought hither the capital of older states and nations to spread over the wheat lands of the great northwest a vast network of railways in advance of population. The faith which opened these vast lines of travel has not been vain. Year by year an increasing, ever-augmenting tide of immigration, chiefly of North European peoples, sober, austere, virtuous and deeply religious, has been flowing past our doors to settle on the prairies — God's reservation for the free and the poor. Such a movement of population has not been seen since the days of Theodoric. Towns and villages by hundreds spring up as if by magic, and, yearly, millions of acres of virgin soil spread their golden harvests to the unaccustomed heavens.

The same apparatus which distributes immigration follows it up with those supplies which civilized man cannot forego, and returns to the markets of the world the products of the territory occupied. To intercept at this point the flow of cereals from the wheat lands, to assemble here the multitudinous drives of pine logs banked each winter in the Mississippi pineries and by means of the power of these falls to convert them into food and building materials, no philosopher, economist or legislator was needed to suggest. The voluntary principle was all-sufficient, and already for years the rumble of the flour mill, the buzz of the circular saw and the keen whirr of the planer have drowned the thunder of the Father of Waters.

Under these circumstances, Mr. President, you will not, I am sure, look for the finish and elegance of the older and forwarder cities. It would be impossible for us, after the fashion of the spry housewife, to spring about and suddenly "tidy up things" for your visit. The bricks and mortar, the stone and timber which bar or embarrass your passage cannot be removed till they are built into the structures whose foundations you see on your right hand and on your left.

But I should do a wrong to my city were I to leave with you the impression that we are so overwhelmed and engrossed with our material labors

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as to have no care for things of the mind and the higher life. If that were true, why should we welcome with so much sincere ardor the assembling of your association? From the villages of New England and the farmhouses of the middle states our people have brought that perennial curiosity, that thirst for knowledge, that intense though sombre imagination, which have given American civilization and American literature a cast and hue of their own. I must, in a word, praise our system of public schools, both city and state, which under able management and a steady popular support cannot, we believe, be ranked below those of any community of our size in the Union. Minnesota is the first state which has organized its secondary as well as its primary and superior education, and offers to every child in the state a free course of studies, from the alphabet to the degree of master of arts. Our churches, goodly in size and number, may speak for the interests of religion. The future will attest the diligence and the fidelity of those who love music and the sister arts, of whom our older cities might be proud.

It is thus, however, Mr. President, that we Minneapolitans, alert, preoccupied, pause in the midst of our labors to welcome your already venerable association.

We hail you as the survivors of a generation of great investigators, the Sillimans, the Baches, the Henrys, the Morses, the Rogerses, who have made their country famous and their own names as imperishable as science herself. We hail you as the worthy successors of such a generation, perpetuating and enlarging their work.

In common with civilized people we recognize the immense debt of the modern world to science; yet often no doubt when we are filling the sky with applause to some lucky inventor we are not remembering the years, perhaps generations, of inconspicuous and painful labors, carried on in dim studies and laboratories which made the invention possible. Let the inventor have his glory and his profit without envy and without stint, but let us not forget to build the cenotaph of a thousand nameless geometers, star-gazers and natural philosophers, who working in silence and obscurity, without thought of fame or hope of reward, put it in his power to bless and captivate the world.

We are grateful, therefore, to science for the telescope and the microscope, for chloroform, for the photograph, for all the marvellous applications of electricity. To science we owe that magnificent apparatus of transportation which is the crowning and distinctive feature of modern material life. By its means men and goods flow in vast masses up and down and around the world. These movements are like to the currents of the ocean, the flow of mighty rivers and the majestic march of the heavenly bodies. To science we owe the thousand appliances which yield comfort and even elegance to the humblest household.

Immense as are these contributions of science to material comfort and happiness, she has, I still think, performed greater services to mankind. The scientific method developed in the study of nature has spread to all branches of investigation. It has permeated all our education, it has

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boldly leaped the boundary between physics and metaphysics. It has even penetrated into industry and business and common life. The modern man first collects what knowledge he can about his enterprise or adventure and assures himself of their value. He then makes the best guess he can in regard to the future. Then he assembles new facts and as the facts require, amends his theory, till at length it becomes a working rule, maxim or principle. He knows not merely how to know but how to guess. The penetration of the scientific method into the operations of trade in great commercial cities is very conspicuous. We endeavor to gamble scientifically. No Drew or Armour or Gould ever forms his corner without a most careful study of the situation, and his venture is his bet on the correctness of his theory. The further extension of the scientific method till it shall become the guide of conduct in the every-day life of all men, is now a chief problem in education.

In the next place I think science may at length fairly claim to have wrought out under great difficulties a working hypothesis of our universe in the nebular hypothesis and its almost necessary corollary, "evolution." It cannot be denied that we are all in some sense evolutionists, some of us against our prepossessions, some of us by insensible but progressive lapses. I am not competent to argue on this great theme. But I feel bound to admit that the evolution doctrine in one form or another has quietly taken possession of the modern mind. Why may we not gladly accept it as a most useful working hypothesis of the mode of creation? I say of the mode of creation, for that mystery of creation itself will forever mock the powers of man. Only this we know, that unless human consciousness is a juggle and human language a mockery, there can never be to man a creation without a creator, nor an evolution without an evolver.

Another great service of science is the maintenance in the world of a body of men, a lay priesthood, devoted to the search for truth for its own sake and its own value. In a mercenary age, when in the opinion of a distinguished contemporary, mercantilism has become a disease and excrescence on society, the example of such a body of men is of supreme value in the training of the new generations. Youths are formed, a wise Greek has taught us, not so much by schools as by the example of distinguished men. To welcome you as a body of earnest lovers of and seekers after truth, from the love of it and your kind, would be well worth our while, were it our only motive to impress and inspire the children and youth of our city. In doing you honor we give them a lesson no books nor masters can impart. For their sake we then renew our welcome.

A still greater benefit of science to mankind is the emancipation it has wrought for us in the last generations from superstition and the dominion of imaginary powers. It is no long time since it was generally believed by civilized men that human affairs were under the control of the spirits of the air, good or evil. Men walked in cringing terror by day and night, of demons and goblins damned. The earthquake, the tornado, the lightning's stroke, they looked upon as instruments of punishment for the sins

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of rulers and peoples. Thanks to science the modern world has emerged from this cloud of gloom.

We have some certain knowledge. Our knowledge is not merely qualitative, but quantitative. Truth ever makes free. Above all, we know that all things in nature are governed by law — law, "whose seat is the bosom of God, whose voice is the harmony of the world." The beautiful conception of the Greeks of the universe as a kosmos, that is, an embodiment of divine and all-pervading order, is the grand presupposition of modern thought. We now know that the phenomena of nature have no relation to human conduct, the impartial rain falling alike on just and unjust. Men walk the earth erect and free, fearing no bogies nor demons. How vast and how blessed the relief to childhood!

In dispelling superstition, science has incidentally wrought her greatest services to mankind in the purification of religion. The time is coming when grateful thanks will be rendered by the ministers of religion for the emancipation which science has wrought for the faith, and then the conflict of science and religion will only be remembered as the antagonism of crude theories on the one hand and cruder superstitions on the other.

Grateful we are for all the knowledge which science has collected and collated, and perpetuated to our use. All honor to the men who are consecrated to truth in her service. We may not know what marvels, far surpassing all the gifts of the past, the science of the future may reveal. Still we must remember that the human mind is finite, while the truth is infinite. The vast unknown engirdles our little circle of light. The mystery of life and death no son of earth has ever penetrated. Welcome all truth, all knowledge then! welcome to the faith which points to the continuance of life in a land where study will be no weariness to the soul, where no veil of flesh will cloud the vision, where science and religion shall be forever one, where men shall know even as they are known!

To the above addresses of welcome President YOUNG responded as follows:

Gentlemen: On behalf of my fellow-members of the association, I return you our sincerest thanks for the hearty welcome we have received to this magnificent state, this young and beautiful city, this vigorous, energetic, warm-hearted community. When you first invited us here it was not in our power to come, but your second invitation we have accepted most gladly, and hope and believe that our meeting here will prove a benefit and pleasure to all concerned.

Some of us have known you personally before, and most of us have long been more or less familiar at second hand with your state and city, and yet I think to many of us it is something like a new revelation to see for ourselves what a few years have accomplished. I am not enough of a Latin scholar to quote my Virgil well, but I have been all the time most forcibly reminded of the passage in which Æneas first comes in sight of rising Carthage—most emphatically "the work boils here." We see no drones or sluggards, but every shoulder is at the wheel and everything is moving.

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It may perhaps seem to you sometimes, when in our sectional meeting we discuss some question about the stars, or some hypothesis as to the formation of rock strata, or the structure of some worm or insect, that we are not of the current of life, and contribute nothing to the advancement of the world. But you *know* it is not so, and your invitation to hold our meeting here shows that you know it.* The world advances not on one line only, but on many; on lines material, intellectual, spiritual. To some extent the movements are indeed independent, but not very far. Any true advance on either line implies corresponding movement on each of the others if not absolutely simultaneous yet swiftly consequent. There is no need to ask you here how much this city owes to modern science, when I see on every side, in all your streets and storehouses and mills the practical application of the highest engineering, mechanical and electric art. And in the future it is almost certain that science is to contribute still more liberally to business. But not mainly for this reason do I claim your regard to science, but because, made in the image of God as we are, knowledge and understanding are as truly wealth and power as lands and food and money.

I need not add that as you have invited us here, so we on our part most cordially invite you to attend all our meetings and listen to our papers and their discussion. We cannot promise that every paper will be interesting to all, but each one I think will be able to select certain ones he will be glad to hear. And if any one of you choose to join us and enroll yourselves as promoters of the advancement of science, our membership is open on easy terms.

Once more, gentlemen, we thank you for your cordial welcome and address ourselves to our business, in hope and confidence that our meeting here is to be in the highest degree pleasant and successful.

The regular business of the General session was then taken up.

The GENERAL SECRETARY read a list of *seventy-three* persons recommended by the Standing Committee for membership in the Association and they were duly elected.

The PERMANENT SECRETARY read the financial report of the Association.

The PERMANENT SECRETARY also read the following list of twenty members, notices of whose death had been received during the past year.

STEPHEN ALEXANDER, of Princeton, N. J., past president. Joined the Association at the first meeting. Born Sept. 1, 1806. Died June 25, 1883.

LOUIS AUFRECHT, of Cincinnati, Ohio. Joined at the 30th meeting.

Sir REDMOND BARRY, of Melbourne, Australia. Joined at the 25th meeting.

G. W. BELFRAGE, of Clifton, Texas. Joined at the 29th meeting. Died Dec. 7, 1882.

ROBERT BRIGGS, of Philadelphia, Pa. Joined at the 29th meeting.

HORACE BROWN, of Salem, Mass. Joined at the 27th meeting. Died July, 1883.

L. S. BURBANK, of Woburn, Mass. Joined at the 18th meeting.

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PAUL A. CHADBOURNE, of Amherst, Mass. Joined at the 10th meeting. Born Oct. 21, 1823. Died Feb. 23, 1883.

Miss MARGARET S. CHENRY, of Jamaica Plain, Mass. Joined at the 29th meeting. Died Sept., 1882.

JOSIAH CURTIS, Washington, D. C. Joined at the 18th meeting. Died Aug. 1, 1883.

HENRY DRAPER, of New York, N. Y. Joined at the 28th meeting. Born, 1837. Died in 1883.

O. H. FITCH, of Ashtabula, Ohio. Joined at the 7th meeting. Died Sept. 17, 1882, in his 80th year.

A. P. GARBER, of Columbia, Pa. Joined at the 29th meeting. Died Aug. 26, 1881.

G. S. B. HEMPSTEAD, Portsmouth, Ohio. Joined at the 29th meeting. Died July 9, 1883, in his 89th year.

JAMES ORTON, Poughkeepsie, N. Y. Joined at the 18th meeting. Died Sept. 24, 1877.

WILLIAM SCHLEY, of New York, N. Y. Joined at the 28th meeting. Died in 1882.

JOSEPH S. St. JOHN, of Albany, N. Y., acting General Secretary of the 7th meeting. Died Nov. 23, 1882.

A. F. TAYLOR, Cleveland, Ohio.

THOMAS W. TOBIN, of Louisville, Ky. Joined at the 30th meeting. Died Aug. 4, 1883, in his 41st year.

R. S. WILLIAMSON, of San Francisco, Cal. Joined at the 12th meeting.

President YOUNG gave a brief account of the life of Prof. Stephen Alexander of Princeton, N. J., who was an original member of the Association and was President in 1859.

After formal announcements by the PERMANENT SECRETARY and officers of the LOCAL COMMITTEE the General Session adjourned.

On the adjournment of the General Session the SECTIONS met in their respective section rooms to complete their organizations with the following result.

SECTION A.

Vice President: W. A. ROGERS.
Secretary: G. W. HOUGH.
Member of Standing Committee: E. S. HOLDEN.
Sectional Committee: J. R. EASTMAN, H. T. EDDY, E. W. HYDE.
Member of Nominating Committee: S. J. COFFIN.
Sub-committee on Nominations: A. B. NELSON, C. J. SMITH, E. FRISBY.

SECTION B.

Vice President: H. A. ROWLAND.
Secretary: F. E. NIPHER.
Member of Standing Committee: LE ROY C. COOLEY.
Sectional Committee: T. F. JEWELL, W. A. ANTHONY, H. S. CARHART.
Member of Nominating Committee: H. R. GOODNOW.
Sub-committee on Nominations: T. C. MENDENHALL, C. F. BRACKETT.

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SECTION C.

Vice President: E. W. MORLEY.
Secretary: W. MCMURTRIE.
Member of Standing Committee: C. LEO MEES.
Sectional Committee: W. G. MIXTER, R. B. WARDER, A. P. S. STUART.
Member of Nominating Committee: A. P. S. STUART.
Sub-committee on Nominations: A. SPRINGER, H. H. DINWIDDIE, J. A. DODGE.

SECTION D.

Vice President: DE VOLSON WOOD.
Member of Nominating Committee: C. M. WOODWARD.
Sub-Committee on Nominations: T. R. BAKER, L. L. MCINNIS, W. A. PIKE.

SECTION E.

Vice President: C. H. HITCHCOCK.
Secretary: A. A. JULIEN.
Member of Standing Committee: E. W. CLAYPOLE.
Sectional Committee: J. W. SPENCER, T. C. CHAMBERLAIN, W. UPHAM.
Member of Nominating Committee: N. H. WINCHELL.
Sub-committee on Nominations: E. ORTON, J. F. JAMES, J. W. POWELL.

SECTION F.

Vice President: W. J. BEAL.
Secretary: S. A. FORBES.
Member of Standing Committee: O. S. WESTCOTT.
Sectional Committee: C. E. BESSEY, W. R. LAZENBY, E. D. COPE.
Member of Nominating Committee: H. OSBORN.
Sub-committee on Nominations: D. S. KELLICOTT, J. M. COULTER, W. G. FARLOW.

SECTION G.

Vice President: J. D. COX.
Secretary: W. H. WALMSLEY.
Member of Standing Committee: W. LILLY.
Sectional Committee: W. J. BEAL, T. C. MENDENHALL, W. A. ROGERS.
Member of Nominating Committee: C. V. RILEY.
Sub-committee on Nominations: W. H. WALMSLEY, C. LEO MEES, D. S. KELLICOTT.

SECTION H.

Vice President: O. T. MASON.
Secretary: G. H. PERKINS.
Member of Standing Committee: E. S. MORSE.
Sectional Committee: P. R. HOY, F. W. PUTNAM, N. S. TOWNSHEND.
Member of Nominating Committee: NATHANIEL PAINE.

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SECTION I.

Vice President: FRANKLIN B. HOUGH.

Secretary: JOSEPH CUMMINGS.

Member of Nominating Committee: C. W. SMILEY.

Sub-committee on Nominations: J. R. DODGE, WM. W. FOLWELL, JOHN HICKS.

The addresses of the Vice Presidents were given as follows:—

Vice President		ROGERS	before	Section	A	at	2.30	P.	M.
"	"	HITCHCOCK	"	"	E	"	2.30	"	"
"	"	HOUGH	"	"	I	"	2.30	"	"
"	"	ROWLAND	"	"	B	"	4	"	"
"	"	BEAL	"	"	F	"	4	"	"
"	"	MASON	"	"	H	"	4	"	"

No addresses were given by the Vice Presidents of Sections C, D, and G.

The address of the retiring President, Dr. J. W. DAWSON, was given at 7.30 P. M. in the Westminster Church.

After the address of Dr. Dawson the LOCAL COMMITTEE gave a reception to the Association in the parlors of the Nicollet House.

THURSDAY, AUGUST 16.

The Association met in GENERAL SESSION at 10 A. M., President YOUNG in the chair.

The PRESIDENT announced that Section C would hereafter meet in Room 39 on the second floor of the University and that a meeting of Botanists would be held in the room of Section F at 2 P. M. on Thursday.

The LOCAL COMMITTEE gave notice that arrangements had been made to enable members of the Association to visit the lumber and flouring mills of Minneapolis on Saturday morning.

The same committee also announced an excursion for members of the Association on Saturday to Lake Park Hotel and Lake Minnetonka, via the Minneapolis and St. Louis railroad, leaving Minneapolis at 11 A. M.

A list of twenty-nine persons which the Standing Committee had recommended for membership in the Association was then read by the GENERAL SECRETARY and they were duly elected.

The following communications were read by the General Secretary.

NORTHWESTERN DIVISION ERIE TELEPHONE AND TELEGRAPH CO.
MINNEAPOLIS, AUGUST 14, 1883.

PROF. N. H. WINCHELL, Secretary of the Local Committee for the Minneapolis Meeting:

DEAR SIR: During the session of your Association we shall be pleased to extend the service of our local exchange to members and guests and would appreciate an inspection of our operating department by any of your people.

Yours truly,

ARTHUR S. TEMPLE,
General Superintendent.

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YOUNG MEN'S CHRISTIAN ASSOCIATION,
MINNEAPOLIS, MINN., AUG. 9, 1883.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

GENTLEMEN: We very cordially invite you to visit and inspect our new quarters in the Syndicate Block 519-521 Nicollet Avenue, at your leisure during your sessions in our city. Our rooms are open daily to visitors from 9 A. M. to 9.30 P. M. The Secretary, or his assistant, will cheerfully give information or show any attention within his power.

Yours truly,
H. E. FLETCHER, Pres.,
JOHN H. ELLIOTT, Gen. Secretary.

The General Session then adjourned.

FRIDAY, AUGUST 17.

The Association met in General Session at 10 A. M., the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of thirty persons recommended by the Standing Committee for membership in the Association and they were duly elected.

The following communication was read by the GENERAL SECRETARY

ST. PAUL, MINN., AUGUST 15, 1883.

TO THE PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DEAR SIR: At the Meeting of the Board of Directors of the St. Paul Chamber of Commerce held on the sixth instant, a resolution was adopted, appointing the gentlemen whose names are given below, looking to the entertainment of your Association in St. Paul. We sincerely trust that you will give us the pleasure of showing you our city and beg you will name a day for that purpose.

Respectfully,
R. O. SWEENY, Chairman Special Committee,
C. A. MCNEALE, Secretary.

Committee on Entertainment, Messrs. R. O. SWEENY, H. N. ELMER, F. A. FOGG, H. A. CASTLE, L. W. RUNDLETT.

By a vote of the Association the invitation was accepted for Monday afternoon August 20; the members of the Association to leave East Minneapolis at two o'clock.

Several announcements were made by the Local Committee.

In accordance with the vote of the STANDING COMMITTEE, the paper by Dr. T. STERRY HUNT of Montreal, on a Classification of the Natural Sciences, was read, after which the General Session adjourned.

On Friday evening, Prof. F. W. PUTNAM of Cambridge, Mass., gave a lecture illustrated by diagrams and lantern pictures, on "Altar Mounds

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and their contents," before a large and highly interested audience in the Church of the Redeemer. The lantern was furnished by Dr. E. P. HOWLAND of Washington.

SATURDAY, AUGUST 18.

No meetings of the Association were held to-day. During the morning many members of the Association availed themselves of the privilege of visiting the lumber and flouring mills and found much to admire in the wonderful mechanical improvements which astonished them at every turn.

At 11 A. M. about four hundred members of the Association and guests took an extra train on the Minneapolis and St. Louis R. R. for the Lake Park Hotel at Lake Minnetonka. On arriving at the hotel they were furnished with an excellent dinner by the LOCAL COMMITTEE. After dinner the party embarked on the steamer City of St. Louis, and made the grand tour of Lake Minnetonka. Most of the party landed at Chapman's, and were highly edified by speeches from Mr. D. BLAKELY, the Hon. W. D. WASHBURN and the Hon. IGNATIUS DONNELLY. Returning by the same route the excursionists reached Minneapolis about 6 P. M. highly pleased with the zeal and skill of the Local Committee, and the beauties of Minnetonka.

MONDAY, AUGUST 20.

The Association met in GENERAL SESSION at 10 A. M. the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of twenty-three persons recommended by the STANDING COMMITTEE for membership in the Association, and they were duly elected.

The PRESIDENT announced that the reports of Special Committees were now in order.

The "Committee on Weights, Measures and Coinage," made no report and was continued.

The "Committee to Memorialize the Legislature of New York for a new survey of Niagara Falls" made no report and was discharged.

The "Committee on the best methods of Science Teaching in the Public Schools" reported progress and was continued.

The "Committee on the Registration of Marriages, Births and Deaths," made no report and was continued.

The "Committee on Standard Time" reported that the Railway companies had taken such action as left little for the Committee to do. The committee was discharged.

The "Committee on Stellar Magnitudes" reported progress and was continued.

The "Committee on State Geological Surveys" made no report and was discharged.

The "Committee to confer with committees of Foreign Associations for the Advancement of Science, with reference to an International Convention of Scientific Associations," reported progress and was continued.

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The "Committee to confer with a Committee of the British Association for the Advancement of Science on the Annual Records of Science" made no report and was discharged.

The "Committee on indexing the Literature of the Chemical Elements reported through its Chairman that a paper reporting progress would reach the Association before the day fixed for adjournment. The Committee was continued. [The report was received and is printed on p. 147 of this volume.]

The "Committee on a Primary Meridian and an International Standard of Time" made no report and was discharged.

The "Committee on International Congress of Geologists" reported progress, and was continued with power to add to its number.

The "Committee to confer with the United States Geologist in regard to coöperation between the Government and State Geological Surveys," reported progress and was continued with power to add to its number.

The following Constitutional amendment proposed at the Montreal Meeting was then read and adopted by a vote of the Association.

Add to ARTICLE 7 of the Constitution the following:—

"The Standing Committee shall have power to exclude from the Association any Member or Fellow, on satisfactory evidence that said Member or Fellow is an improper person to be connected with the Association, or has in the estimation of the Committee made improper use of his membership or fellowship."

The following report of the Committee on interchange of courtesies between the American and the British Associations for the Advancement of Science, in 1884, was read by the GENERAL SECRETARY.

The Committee report the following recommendations:—

That a cordial invitation be extended to the members of the British Association to attend the meeting of this Association, and take part in its scientific proceedings.

That in order to facilitate the exchange of courtesies, the meeting of this Association for 1884 begin on September 3. [Afterwards changed to Thursday, Sept. 4.]

That the committee on the Berlin Congress of Geologists be requested to correspond with the officers of the Congress and of the British Association with the view of arranging for such a date as may be suitable to the delegates from the two Associations.

That a committee be continued to attend to such matters as may arise in connection with the meetings of the Associations.

The Association voted to accept the report and to adopt the recommendations, leaving the date of the meeting to be recommended by the Nominating Committee.

The following committee was appointed and continued in accordance with the report: C. A. YOUNG, J. P. LESLEY, J. W. DAWSON, T. STERRY HUNT, E. S. MORSE, T. C. MENDENHALL, F. W. PUTNAM.

The report of the Committee appointed to consider the question of duty

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on scientific books, was then read. On motion of Hon. J. D. Cox it was voted that a committee of five be appointed in accordance with the recommendations of the report to present the matter to Congress.

The following committee was appointed: H. A. ROWLAND, C. A. YOUNG, J. W. POWELL, J. D. COX, E. S. MORSE.

The following report of the Committee on Privileges of Members of the Association, with respect to meetings, was read, and adopted by vote of the Association.

That every member of the Association shall have the privilege of registering members of his family, not including men over twenty-one years of age, by paying the sum of three dollars for each person so registered, and shall receive for them specific badges which will entitle the holder thereof to such privileges as may be extended to the members generally by the local committee for the meeting.

The NOMINATING COMMITTEE were notified to meet at the Nicollet House at 9.30 P. M. Monday.

After several announcements by the LOCAL COMMITTEE a paper on The Evidence for Evolution, in the History of the Extinct Mammalia, was read by Prof. E. D. COPE of Philadelphia.

On the completion of Prof. Cope's paper the General Session adjourned.

MONDAY AFTERNOON.

Soon after lunch those members of the Association who accepted the invitation to visit St. Paul took a special train of Chicago, Milwaukee and St. Paul R. R. cars for the excursion. The train stopped at Minnehaha Falls giving ample time to see this beautiful and historic sheet of water, and after several considerable delays the party arrived safe in St. Paul where they took carriages provided by the citizens' committee and drove to the State House. Here, speeches of welcome were made by Mayor C. D. O'BRIEN of St. Paul, Col. EDWARD DANIELS of St. Paul and Governor L. F. HUBBARD. Responses on the part of the Association were made by Pres. C. A. YOUNG, Gen. J. D. COX and Major J. W. POWELL.

The unexpected delay of the train had deranged many of the plans of the St. Paul reception committee, but most of the party were able to see a large portion of the city and reach the Metropolitan and Merchants' Hotels in season for the excellent supper provided by the reception committee. At 8.30 P. M., the return train left St. Paul and soon reached Minneapolis without delay or accident.

On Monday evening, at Plymouth Church, an illustrated lecture on The Fish and Fisheries of the United States, was given by Mr. C. W. SMILEY of the Smithsonian Institution. The lantern illustrations were by Dr. E. P. HOWLAND of Washington, D. C.

TUESDAY, AUGUST 21.

The Association met in GENERAL SESSION at 10 A. M., the PRESIDENT in the chair.

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A list of twenty persons recommended by the STANDING COMMITTEE for membership in the Association was read by the GENERAL SECRETARY and they were duly elected.

The following list of members recommended by the STANDING COMMITTEE to be Fellows of the Association was then elected by ballot.

Abbott, Dr. Chas. C., Trenton, N. J. (29).
Anderson, Rev. Joseph, Waterbury, Conn. (29).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Barnard, Edward E., Nashville, Tenn. (26).
Bartlett, Prof. Edwin J., Dartmouth Coll., Hanover, N. J. (28).
Bausch, Edward, Rochester, N. Y. (26).
Billings, Dr. John Shaw, Washington, D. C. (32).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Burr, Prof. William H., Troy, N. Y. (31).
Carpmael, Charles, Director of Magnetic Observatory, Toronto, Can. (31).
Case, Col. Theo. S., Kansas City, Mo. (27).
Chandler, Prof. Chas. Henry, Ripon, Wis. (28).
Clapp, Miss Cornelia M., Mt. Holyoke Seminary, South Hadley, Mass. (31).
Crandall, Prof. A. R., Lexington, Ky. (29).
Curtis, Dr. Lester, 1558 Wabash avenue, Chicago, Ill. (29).
Davenport, B. F., M. D., 751 Tremont St., Boston, Mass. (29).
Dorsey, Rev. Jas. O., Bureau of Ethnology, Washington, D. C. (31).
Dudley, Prof. Wm. R., Ithaca, N. Y. (29).
Eaton, Hon. John, U. S. Commiss'r of Education, Washington, D. C. (25).
Fairchild, H. L., 153 E. 47th St., New York, N. Y. (28).
Fernald, Prof. M. C., State Agricultural College, Orono, Me. (22).
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29).
Fletcher, James, Library of Parliament, Ottawa, Can. (31).
Freeman, Spencer H., Cleveland, Ohio (29).
French, Prof. Thomas, jr., Urbana, Ohio (30).
Gallaudet, Edw. M., Pres. Nat. Deaf Mute College, Washington, D. C. (28).
Gray, Dr. Elisha, Highland Park, Ill. (32).
Gulley, Prof. Frank A., Agricultural College, Oktibbeha Co., Miss. (30).
Hall, Prof. C. W., Univ. of Minnesota, Minneapolis, Minn. (28).
Halsted, Byron D., 245 Broadway, New York, N. Y. (29).
Hanaman, C. E., Troy, N. Y. (19).
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28).
Holman, Silas W., Mass. Inst. of Technology, Boston, Mass. (31).
Holmes, Wm. H., Smithsonian Institution, Washington, D. C. (30).
Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30).
Hotchkiss, Jed., Staunton, Va. (31).
Hovey, Rev. H. C., New Haven, Conn. (29).
Jordan, Prof. David S., Bloomington, Ind. (31).
Kellicott, David S., Buffalo, N. Y. (31).
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29).
Kinnicutt, Dr. Leonard P., Worcester, Mass. (28).

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Kunz, G. F., care Tiffany & Co., New York, N. Y. (29).
Landreth, Prof. Olin H., Vanderbilt Univ., Nashville, Tenn. (28).
Larkin, Edgar L., New Windsor, Mercer Co., Ill. (28).
Mackintosh, James B., School of Mines, Columbia Coll., New York (27).
Moses, Prof. Thos. F., Urbana University, Urbana, Ohio (25).
Osborn, Henry F., S. D., Garrisons, N. Y. (29).
Payne, Prof. Wm. W., Carleton College, Northfield, Minn. (30).
Pickering, Wm. H., 84 Mt. Vernon St., Boston, Mass. (29).
Robinson, Prof. S. W., Univ. of Ohio, Columbus, Ohio (30).
Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28).
Rosenspitz, Dr. Alexander, Rabbi, Lock Box 430, Portland, Oregon (26).
Smiley, Charles W., Smithsonian Institution, Washington, D. C. (28).
Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30).
Upton, Winslow, Army Signal Office, Washington, D. C. (29).
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28).
Webb, Prof. J. Burkitt, Ithaca, N. Y. (31).
Wheeler, Prof. C. G., Univ. of Chicago, Chicago, Ill. (18).
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31).

The PRESIDENT announced that the time had arrived for selecting the place of meeting for 1884 and that the NOMINATING COMMITTEE had recommended the city of Philadelphia.

The following invitation from men of science and from corporate bodies in Philadelphia was then read:—

PHILADELPHIA, AUGUST 1, 1883.

TO THE PRESIDENT, OFFICERS AND MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

GENTLEMEN: The undersigned being severally authorized to represent the institutions, corporations, etc., set opposite their names, or joining heartily with the former in their individual capacity as citizens of Philadelphia, do hereby cordially invite your honorable body to hold the thirty-third session of the American Association for the Advancement of Science, for 1884, in the city of Philadelphia.

Prof. H. Carvill Lewis, Prof. Edward D. Cope, Dr. Persifor Frazer, Prof. Angelo Heilprin, Rev. Henry C. McCook, D. D.; *Committee appointed by the Academy of Natural Sciences of Philadelphia.*

Daniel G. Brinton, M. D., Prof. George F. Barker, M. D., Prof. J. Peter Lesley, Henry Phillips, jr., Wm. Morris Davis, Esq.; *Committee appointed by the American Philosophical Society.*

Wm. P. Tatham, Esq., Alex. E. Outerbridge, jr., Prof. Edwin J. Houston, Coleman Sellers, Esq., Wm. H. Wahl, Ph. D.; *Committee appointed by the Franklin Institute.*

Prof. Wm. Pepper, M. D., Provost of the University, Prof. Joseph Leidy, M. D., President of the Acad. of Nat. Sciences, S. Weir Mitchell, M. D., Hon. John Welsh, William Sellers, Esq.; *On behalf of the University of Pennsylvania.*

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Isaac Lea, LL. D., John L. LeConte, M. D., Prof. Geo. F. Barker, M. D.; *Ex-Presidents A. A. A. S.*

Hon. Samuel G. King, *Mayor of the city of Philadelphia.*

Hon. Wm. Henry Lex, *President of the Common Council of Philadelphia.*

Edward T. Steel, Esq., *President of the Board of Education.*

Prof. L. M. Haupt, Thos. C. Clarke, Esq., Frederic Graff, Esq., Strickland Kneass, Esq., Rudolph Hering, Esq.; *Committee appointed by the Engineers' Club of Philadelphia.*

Henry C. Lea, Esq., J. G. Rosengarten, Esq., Wm. A. Ingham, Esq., Thos. Cochran, Esq., Edward R. Wood, Esq.; *On behalf of the Philadelphia Social Science Association.*

Right Rev. Wm. Bacon Stevens; *Bishop of Pennsylvania, President of the Board of Trustees of the Philadelphia Divinity School, President of the Protestant Episcopal Hospital, President of the University Club.*

Horace Howard Furness, Esq., *President of the Penn Club.*

Prof. Roberts Bartholow, M. D., LL. D., *Dean of the Faculty of the Jefferson Medical College of Philadelphia.*

Prof. Edward H. Magill, *President of Swarthmore College.*

Prof. Geo. J. Ritchè, *President Central High School.*

Prof. A. H. Fetterolf, *President,* Prof. H. D. Gregory, *Vice-President, Girard College.*

Prof. C. N. Peirce, D. D. S., *Dean of the Pennsylvania College of Dental Surgery.*

James L. Claghorn, Esq., *President Pennsylvania Academy of Fine Arts, President of the School of Design.*

John L. Le Conte, M. D., *President American Entomological Society.*

Lloyd P. Smith, Esq., *Librarian, Library Co. of Philadelphia.*

T. Morris Perot, Esq., *President of the Mercantile Library Co.*

George H. Boker, *President,* and Samuel B. Huey, *Secretary of the Union League of Philadelphia.*

Thomas Chase, LL. D., *President of Haverford College.*

George W. Childs, Esq., *Editor of the Public Ledger.*

Hon. A. W. Tourgee, Esq., *Editor of the Continent.*

Col. A. Loudon Snowdon, *Superintendent U. S. Mint.*

Gen. H. S. Huidekoper, *Postmaster at Philadelphia.*

Prof. F. V. Hayden, M. D., LL. D., *U. S. Geological Survey.*

Franklin B. Gowen, Esq., *President,* and Albert Foster, Esq., *Secretary of the Philadelphia and Reading Railroad Company.*

John C. Sims, jr., Esq., *Secretary Pennsylvania Railroad.*

A. J. Drexel, Esq., John Wanamaker, Esq., Geo. Whitney, Esq., Moro Phillips, Esq., James A. Wright, Esq.

The PRESIDENT said that a formal invitation had been received from New York but considering the prior action of the Philadelphia people the New York claim was not urged.

A vote was taken on the selection of the place of meeting in 1884, and Philadelphia was chosen unanimously.

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The time of meeting was fixed for the third of September, with the proviso, that the retiring and incoming Presidents and Permanent and General Secretaries for 1883 and 1884 should be empowered to change the date if they deem it desirable. [This committee has found it desirable to change the date to Thursday, Sept. 4.]

The GENERAL SECRETARY read the list of Fellows recommended by the Nominating Committee for officers of the meeting to be held in 1884, and they were elected by a unanimous vote of the Association. The list is as follows:—

President: J. P. LESLEY, of Philadelphia.

Vice Presidents: **A** — H. T. EDDY of Cincinnati; **B** — JOHN TROWBRIDGE of Cambridge; **C** — JOHN W. LANGLEY of Ann Arbor; **D** — R. H. THURSTON of Hoboken; **E** — N. H. WINCHELL of Minneapolis; **F** — E. D. COPE of Philadelphia; **G** — T. G. WORMLEY of Philadelphia; **H** — E. S. MORSE of Salem; **I** — JOHN EATON of Washington.

Permanent Secretary: F. W. PUTNAM of Cambridge.

General Secretary: ALFRED SPRINGER of Cincinnati.

Assistant General Secretary: EDWARD S. HOLDEN of Madison.

Secretaries of the Sections: **A** — G. W. HOUGH of Chicago; **B** — N. D. C. HODGES of Salem; **C** — R. B. WARDER of North Bend; **D** — J. BURKITT WEBB of Ithaca; **E** — EUGENE A. SMITH of Tuscaloosa; **F** — C. E. BESSEY of Ames; **G** — ROMYN HITCHCOCK of New York; **H** — W. H. HOLMES of Washington; **I** — C. W. SMILEY of Washington.

Treasurer: WILLIAM LILLY of Mauch Chunk.

After announcements by the LOCAL COMMITTEE the General Session adjourned.

On Tuesday evening the Rev. H. C. HOVEY of New Haven, Ct., delivered an interesting lecture, with lantern illustrations, on Subterranean Scenery of the Great Caverns of the World, to a full audience in Plymouth Church. Dr. HOWLAND also provided the lantern on this occasion.

WEDNESDAY, AUGUST 22.

The Association met in GENERAL SESSION at 10 A. M., the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of eight persons recommended by the STANDING COMMITTEE for membership in the Association, and they were duly elected.

The GENERAL SECRETARY announced that the STANDING COMMITTEE had adopted the following rule in regard to papers presented for reading before the various Sections. "Hereafter no paper will be accepted by the Standing Committee for reading before any of the sections, unless accompanied by such abstract as the writer deems ready for publication."

The report of the "Committee on indexing the Literature of the Chemical Elements" was then read by the GENERAL SECRETARY. [See p. 147.]

The PERMANENT SECRETARY announced that a lady member of the Asso-

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ciation had that morning placed $150 in his hands for the use of the Association. He said that the same generous lady had made a gift of $850 at the last meeting, thus making $1000 in all. These gifts were conditioned on the non-publication of the name of the donor during the meeting, and the Association expressed its gratitude to its modest benefactor by a hearty round of applause.

The PRESIDENT announced that a popular lecture "On the Theory of Evolution in Modern Science," would be given by Prof. E. D. COPE, at the Church of the Redeemer, on Wednesday evening, by request of a number of citizens of Minneapolis.

The regular business of the Session having been completed, Prof. E. T. COX, addressing the Chair, said:

In presenting the following resolution, I desire to say that, only those who have had some experience in providing for the entertainment of the American Association for the Advancement of Science can fully understand the labor and responsibility which rests upon the Local Committee, and I am sure that all will agree with me that these duties have been satisfactorily administered here in this beautiful and marvellously prosperous city of Minneapolis. There has been no confusion or clashing of views, but every effort has been successfully directed to administer to our wants and make our stay as pleasant as possible. Most fully have we enjoyed the generous hospitality so heartily tendered by the Local Committee and citizens, and returning to our homes will bear with us an enduring memory of their kind efforts in our behalf.

Resolved, That the thanks of this Association are hereby tendered to the Local Committee, Hon. GEO. A. PILLSBURY, Prof. N. H. WINCHELL, Dr. ELLIOT and Mr. D. BLAKELY, and to the Citizens of Minneapolis, for the very handsome and generous manner in which they have provided for our comfort and convenience, and for the cordial reception which we have received at their hands.

The resolution was unanimously and enthusiastically adopted.

Prof. CHARLES H. HITCHCOCK presented the following, which was duly endorsed by the Association.

Resolved, That the thanks of the American Association for the Advancement of Science is hereby unanimously tendered to the Regents of the University of the State of Minnesota for the use of their buildings and the appliances connected therewith for the meetings; also to the President, Dr. FOLWELL, for his constant effort to promote the success of our gatherings, and to the other officers of the institution, and especially to Mr. GALLOW, the janitor and carpenter, for his many kind attentions to the officers and members.

Principal DAWSON, of Montreal, after a complimentary allusion to Minneapolis and Lake Minnetonka, offered a resolution of thanks to Hon. W. D. WASHBURN and the Minneapolis & St. Louis Railroad Company for the excursion to the lake, which he characterized as one of the pleasant events in the history of the Association. He said the visit to the Northwest had been a revelation to most of the members, who found that they

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had very inadequate and unworthy conceptions of the great and growing section.

Dr. T. STERRY HUNT complimented the citizens of Minneapolis and of St. Paul on the liberality of their railroad companies, and moved a vote of thanks to Gen. W. D. WASHBURN for the use of the steamer St. Louis for the excursion on Lake Minnetonka and to all the companies who had extended so many and so acceptable favors to the Association. He intended to include all the companies, but there were so many of them that he would crave pardon for not knowing all their names. Dr. HUNT made several complimentary allusions to St. Paul and warmly thanked the citizens for the reception tendered by that city.

The vote of thanks was unanimously given.

Dr. STEPHEN D. PEET made a happy allusion to the race between electricity and thought, which had been brought on by the wonderful achievements in the "chaining of lightning," and concluded by offering a vote of thanks to the Telegraph and Telephone companies for their contributions to the convenience of the members.

The vote was heartily given.

Prof. C. V. RILEY, after some complimentary remarks in regard to the ability and vigor of the whole press of the cities of Minneapolis and St. Paul, and of the accuracy manifested in their printed reports of this meeting, presented the following resolution which was unanimously adopted.

Resolved, That the thanks of the Association are especially due to the Tribune of Minneapolis and the Pioneer Press of St. Paul for the carefully prepared reports they have made of the several proceedings of this meeting and of the papers read in the different sections; and to all the newspapers of both cities for the efforts they have put forth in the service of the Association, in forwarding its work and in making its session in Minneapolis alike profitable and agreeable.

THE PERMANENT SECRETARY announced that the Standing Committee had voted copies of the proceedings and papers to each of the academies of science in Minneapolis and St. Paul, to complete their series.

Mr. D. BLAKELY, of the Local Committee, announced that the St. Paul and Duluth Railroad Company and the corporation of Taylor's Falls would give a free excursion and entertainment to such members of the Association as might wish to see the Dalles of the St. Croix. A large number of the members expressed their desire to accept the invitation. On motion of Dr. T. STERRY HUNT the Association passed a vote of thanks to the St. Paul and Duluth Railroad Company, and to the citizens of Taylor's Falls, for their generous invitation.

Mr. GEORGE A. PILLSBURY, chairman of the Local Committee, thanked the Association for the expressions of gratitude and appreciation. He said he hardly thought the Committee deserved the compliments that had been showered upon them. He could say, however, that it had afforded Minneapolis real pleasure to have the honor of entertaining the Association, and that it had been the intention of her citizens to make every

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possible provision for the proper reception of the distinguished guests. We feel that we have failed to fulfil our desire and your expectations. There has been some friction and misunderstanding, and moreover our business men have been so occupied with their pressing enterprises that they have not had time to give to the meetings the attention they deserved and would otherwise have received. We have had no jealous feeling toward St. Paul and would greatly regret that any suspicion of a manifestation of a bitter and unwarranted rivalry should have been aroused. The two cities are really one. Minneapolis is proud of St. Paul and rejoices at her prosperity.

Mr. Pillsbury alluded to the many important matters that are now engrossing the attention of Minneapolitans, mentioning especially the new Sault Ste. Marie railroad project. He said Minneapolis was thoroughly in earnest in this matter and was bound to accomplish the end sought. He concluded by calling on Dr. Wm. W. Folwell to respond further to the resolutions offered.

Professor FOLWELL said on behalf of the Regents and Faculty of the University, that they had simply been glad and proud of the opportunity of placing the buildings, library and collections at the disposal of the Association. They would be content to have every plank in the floors worn out by the tread of the members' feet. To be worn out in good service is the true end of men and materials. He spoke of the faithful labors of Professor N. H. Winchell upon whom as Secretary of the Local Committee had necessarily fallen the main burden of making the arrangements for the meeting, and remarked upon the fine sense of the mover of the resolution, in recognizing the valuable services of the "professor of dust and ashes," Mr. Gallow, on whose behalf he begged to return thanks.

He begged the members not to regard him as speaking these last words in a mere formal, mechanical fashion. During this session many former friendships had been cemented, many new ones formed. All human relations are vain and weak except so far as they rest on a basis of true friendship. Hereafter the people of Minneapolis would remember the members of this Association, not merely as a body of great astronomers, biologists, chemists and geologists, but as a congregation of their warm-hearted happy friends. His parting words in their behalf then were simply, "Dear friends, good-bye."

Prof. N. H. WINCHELL was called for and responded briefly, expressing gratitude for compliments and saying that he hoped the Association felt what it had formally said, that the meeting had been a success. He invited the Association to meet in Minneapolis again after twenty-five years, as it did at Montreal, and promised them that they would see a city of 1,000,000 inhabitants and the University grounds covered with costly buildings.

Gen. NETTLETON followed Prof. Winchell with a word upon the relation of journalism and science. He said he didn't know how the men of science would get the results of their researches before the public, were it not for the press, which presents them to its great audience in interest-

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ing, popular form. The newspaper doubtless gives some misinformation in connection with a great deal of information, but it must be credited with doing valuable service to science. He commented upon the reference made by a member to the cosmopolitan character of the newspapers of the Northwest, and explained it by the fact that the thinking people who had settled here would not tolerate any journalism such as is apt to be found in new sections. He said it seemed out of place for one who did not know a trilobite from a hypothesis to attempt to address such an audience in any capacity, and concluded by emphasizing the remarks made as to the honor which Minneapolis felt had been conferred upon her by the assembling here of the American Association for the Advancement of Science.

Mr. D. BLAKELY of the Local Committee warmly endorsed what his predecessors had said about the feeling of the citizens of Minneapolis. He said St. Paul and Minneapolis were one and indivisible, like the states of the Union, and each ought to glory in the prosperity of the other. He invited the association to come again, and predicted that the members would hardly know the twin cities. This city will then be a Philadelphia, and be able to extend you such a welcome as you expect to get next year.

Mr. THOMAS LOWRY, President of the Street Railway Companies, responded to the resolutions thanking the companies under his direction, in a humorous speech in which he alluded to the rarity of thanks received by the management of street railways, for in his experience complaints were the rule and were presented with the highest zeal, while thanks were the exception and most highly prized.

The PRESIDENT said that the time for closing the sessions of the thirty-second annual meeting of the Association had arrived, and he accordingly pronounced the Association adjourned to meet in 1884 in Philadelphia.

The last excursion in connection with the Minneapolis meeting was made on August 24, by about two hundred and forty members and friends to Taylor's Falls for a view of the Dalles of the St. Croix River. The St. Paul and Duluth R. R. Co. offered the courtesies of their road and the attention and zeal of the officials of that Company, and the cordial hospitality of the citizens of Taylor's Falls combined to make the excursion a fitting close to the enjoyable entertainments so fully extended to the members of the Association.

J. R. EASTMAN,
General Secretary.

NOTE.

By accident the following paragraph was omitted from the Report of the General Secretary for the Montreal Meeting:

Major L. A. HUGUET LATOUR, M.A., N.P., of Montreal, was received at the Montreal Meeting as a duly accredited delegate from the *Φιλολογιχος ευλλογος παρνασσος* (Literary Society, "*The Parnassus*)."

This Society has sent to the Association thirteen scientific books, written by several of its members.

Permanent Secretary.

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REPORT OF THE PERMANENT SECRETARY.

FOR the third time the Association has held a meeting on the western side of the Mississippi, thus acknowledging the claims of the western members and giving its aid to their efforts for the progress of science. As was expected, however, the great distance of Minneapolis from the homes of the majority of the members, and the lack of concessions in fares on the part of most of the railroad companies, made it impossible for many of the members to attend the meeting, and, although the register shows the number in attendance to be twice that at Dubuque or St. Louis, it was by far the smallest attendance at any meeting of late years.

While it must be admitted that all parts of the country have claims upon the Association which should be respected, the question of greatest good to the cause for which it was founded should be carefully considered in accepting invitations from places far away from the residences of the majority of the members; not that large numbers are essential to a successful meeting if the working members and those to whom we look for guidance and encouragement in scientific efforts are present. In this last respect the Minneapolis meeting was a success and the good influence which the meeting exerted upon the young institutions of the rapidly growing city is not to be questioned, while the encouragement it gave to the workers in science in the great Northwest was unquestionably gratifying to them. The only problem to be studied, suggested by the late meeting, is how to bring about a proper appreciation of the work of the Association on the part of the managers of the great railroad lines of the country, by which half rates of fare can be secured for all future meetings; a concession which it seems probable would be granted if the managers of the railroads fully understood the character, objects, and, above all to them, the important influence which the meetings of the Association have on the development of the natural resources of the country and in uniting the people into a strong, coöperative nationality. The recent railroad arrangements and liberal government grants made by Canada, in connection with the coming meeting of the British Association at Montreal, show that these points are appreciated by the Canadian authorities, and it seems hardly possible that they should not be understood on this side of the border. With the great distances to be travelled in this country, it is evident that in order to give the whole country the benefit of the influences of the Association the scientific workers should be able to attend the meetings without too great a drain on their limited means.

The register of the Minneapolis meeting contains 328 names of persons regularly in attendance at the meeting, as follows: From Minnesota 60,

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Illinois 38, Ohio 29, New York 29, Missouri 23, Massachusetts 21, District of Columbia 20, Iowa 18, Pennsylvania 17, Wisconsin 11, Indiana 8, Michigan 7, New Jersey 6, Canada 6, Connecticut 4, Maryland 4, Kentucky 4, Kansas 4, Louisiana 3, New Hampshire 2, Vermont 3, Texas 2, Nebraska 2, Rhode Island 1, Delaware 1, Tennessee 1, Mississippi 1, Alabama 1, Dakota 1, Oregon 1, = 328.

One hundred and eighty-six persons were elected members, of whom one hundred and twenty-nine have accepted and paid their fees, two have declined, and fifty-five have not yet replied to notices sent them.

Fifty-nine members were made fellows, of whom all but eight have accepted.

Since the list was printed in the Montreal volume, notice has been received of the death of 20 members. The names of these, including one original member and past president, are given on p. 476. Since the Minneapolis meeting two other past presidents have died:—Dr. J. L. Le-Conte and Dr. J. Lawrence Smith. Since the Montreal list 22 members have resigned and 2 have been dropped from the list on account of arrearages. During the same period 11 names have been restored, and one patron has been added to the list.

The total number of names now on the roll of members and fellows is 2023, a gain of 101 since the Montreal volume was printed.

With the exception of the vice president elect of Section D, and the secretaries elect of Sections B, D, and I, all the officers were present, as were also three past presidents.

The presidential address, and the addresses of six vice presidents delivered at the meeting, are printed in this volume. The address of vice president of Section I delivered at the Montreal meeting is also printed in this volume.

One hundred and sixty-eight papers were considered by the Standing Committee; of these three were given as public evening lectures with lantern pictures, two were delivered in morning general sessions, four were not accepted, and one hundred and fifty-nine were referred and read in the several sections as follows: A 23, B 19, C 12, D 7, E 35, F 30, G 0, H 23, I 10.

Twenty-four hundred and sixty-two copies of the Montreal volume were printed from the stereotype plates and of these 1733 have been distributed as follows:

To members who have paid the Montreal assessment . .	1500
Sold	25
Distributed by order of the Standing Committee and for exchanges	208
Total number distributed to date . . .	1733

Every effort has been made to get the present volume through the press before June 1, the date assigned by the Standing Committee for its pub-

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lication, and the gain in time over that of previous years leads to the hope that the new rule of the Committee requiring that an abstract ready for printing in the volume shall be received by the Committee before a paper can be referred to a Section for reading, will make possible the still earlier publication of future volumes.

The statement of the receipts and expenditures for the year covering the Montreal meeting, and closing just previous to the Minneapolis meeting, is given in the cash account on the following pages. The general funds of the Association remain as given in the last account with the addition of $150.00 to the Life Membership Fund, and the additional gift of $150.00 to the Reprinting Fund from Mrs. H. Herrman,

Respectfully submitted to the Association,

F. W. PUTNAM, *Permanent Secretary.*

SALEM, MASS., May 14, 1884

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F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

1882–3.

To Balance from last account			$808 57
Assessments previous to 31st Meeting . .	$657 00		
" for " Meeting . .	2,729 00		
" " 32d Meeting . .	1,209 00		
			4,595 00
Admission fees previous to 31st Meeting .	10 00		
" " 31st Meeting	1,005 00		
" " 32d "	60 00		
Fellowship fees	184 00		
			1,259 00
Publications sold and binding	117 53		
Electrotypes of cuts	1 70		
Clerical services	5 00		
Postage, express, and discount on checks .	1 49		
			125 72
General Fund, Estate of Wm. S. Vaux late Treasurer	84 67		
Interest to date	4 23		
			88 90
Life membership commutations	350 00		
Estate of Wm. S. Vaux, late Treasurer . .	56 00		
Interest to date	73 00		
			479 00
Reprinting; gift of members	850 00		
Interest to date	46 30		
			896 30
			$3,252 49

SALEM, MASS., AUGUST 6, 1883.

I have examined the above account and find

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IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE. Cr.

1882–3.

By 2500 copies Proceedings, Vol. 31, Montreal Meeting:—			
Composition, stereotyping and boxes for plates	$1,445 98		
Illustrations	6 19		
Paper and press-work	1,682 83		
Covers, two parts	50 00		
Binding, 2430 copies in paper, two parts	583 20		
" 20 half-morocco, 20.00; 50 in cloth, 25.00	45 00		
Making 25 cloth covers extra	5 00		
Extra copies of addresses, etc.	72 68		
Printing wrappers and wrapping	22 25		
			$3,913 13
Expenses of Montreal Meeting	258 03		
600 copies Montreal list of Members, constitution, etc.	84 80		
Printing for special committee	33 74		
3000 envelopes for special circulars	9 75		
Printing circulars, blanks, cards, etc.	186 55		
Extra clerk hire and labor	16 59		
Travelling expenses Ass't Sec'y	2 60		
Publications bought, and binding	10 95		
Postage, postal cards, stamped envelopes	275 44		
Express, 69.51; Telegraph and Telephone, 1.10	70 61		
Post Office Box to Oct. 1, 1883	8 00		
Postal Guide	1 50		
Minor office expenses	34 24		
Rent of office to July 1, 1883	100 00		
Janitor to July 1, 1883	61 00		
Fuel	19 50		
			1,173 80
Salary of Assistant Secretary	500 00		
Salary of Permanent Secretary	1000 00		
			1,500 00
Plate for reprint Nashville vol.	6 40		
Reprinting fund	889 90		
			896 80
General fund			88 90
Life Membership fund			479 00
Balance to new account			201 86
			$8,252 49

the same correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor.*

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